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THE PSYCHOLOGICAL REVIEW

THE CURVE OF WORK

BY EDWARD L. THORNDIKE

Teachers College, Columbia University

Kraepelin and other students of the changing efficiency of a mental function under continuous exercise have analyzed the gross course of efficiency into certain supposed features or elements. These are the practice effect, the fatigue effect, the 'warming up' effect (Anregung), adaptation (Gewöhnung), initial spurt, end spurt, spurts after fatigue (Ermüdungsantriebe), spurts after disturbance (Störungsantriebe) and the rhythm of attention.

They also often assume that the names thus given to certain changes in efficiency signify adequate causes of the changes. For instance, in distinguishing as 'Gewöhnung,' a rise in efficiency, slower than that which they would call 'Anregung' and less permanent than that which they would call the effect of 'Uebung', they commonly assume that some real thing, Gewöhnung, exists, which is essentially different from the other real things, Anregung and Uebung.

I shall in this paper use these terms only in the former objective meanings of changes in the efficiency of the function, asking, for instance, to what extent initial spurt—a high degree of efficiency appearing in the few minutes of work—is characteristic of work curves in general, or of certain individuals in certain kinds of work.

INITIAL SPURT

This phenomenon is certainly not characteristic of work curves in general. In the case of the 37 work periods of 16

subjects engaged in mental multiplication (of a three-place number by a three-place number)[1] there was no evidence of it. In the case of five adults working at addition[2] (each for four two-hour periods), there is no evidence of it.

Since the results of this second series of experiments will be referred to in several connections, they may best be described now.

Educated adults, graduate students of psychology, worked at adding for 1½ or 2 hours as continuously as possible, recording the time at the completion of each row of 16 examples. 21 different rows, each of 16 examples, were used to avoid memorizing any answers or sequences. The experiment was repeated four times on different days and finally a test of 10 to 15 minutes length was made. Each of the four long series was made under substantially the same external conditions; the fifth test was always made after a full night's rest or more. I append a sample record in Table I. I have turned the gross scores into terms of a single variable by adding five seconds for each error.[3]

I shall present here the results from only five individuals. The results from the others are in agreement with every conclusion which will be stated.

I show in Figs. 1, 2, 3, 4 and 5 the work curves for the five subjects separately.

Since efficiency is measured inversely by the corrected time per row, the lower the curve the greater the efficiency. Since one important fact to be shown by these curves is the variation of the same subject from day to day, the four curves from any one subject are so scaled vertically that the average achievement per unit of time is approximately equal on all four days, and are so scaled lengthwise that each successive fraction of the time is represented by the same abscissa-length on all four days.

I call the reader's attention particularly to the great varia-

[1] The results of which are summarized on pages 75–80 of the *Journal of Educational Psychology*, Vol. II.

[2] The examples were each of 10 one-place numbers, printed in a very clear type 3½ mm. high.

[3] I have also all the results reduced to terms of a single variable by a much heavier penalizing of errors, namely, an addition, per error, of one fifth of the time for 16 examples. All the conclusions to be stated in this paper are supported equally by the results by either method of equating speed and accuracy.

TABLE I

SAMPLE RECORD OF GROSS SCORES

Time taken and errors made in successive rows of 16 ten-digit examples, on four days after approximately the same amount of general mental work and on a fifth day after long rest. Subject *Mc.*

Row	Beginning at 2 P. M., March 10		Beginning at 1:10 P. M., March 11		Beginning at 7:30 P. M., March 12		Beginning at 1 P. M., March 13		Beginning at 9 A. M., March 17	
	Sec.	Err.	Sec.	Err.	Sec.	Err.	Sec.	Err.	Sec.	Err.
1	150	1	138		185	1	172		110	
2	145	1	138		171		120		105	
3	145	1	133		171		110	1	105	1
4	133		122		173	1	119		100	1
5	133		131		172	1	105		105	
6	140		120		125		112	1	108	
7	135	1	115		110		110	1	100	
8	150	1	128		116		112		115	
9	153	2	132	1	130		115		95	
10	142	1	113		111		103			
11	160	2	124		115		118			
12	150	1	126		110		111			
13	140	1	118	1	112	1	105			
14	132		133	1	120	1	120			
15	150	1	135		130	2	127	1		
16	135	1	127	1	113		122	1		
17	157	1	128	1	109		124	1		
18	146		120		135		112			
19	143		133		117	1	115	1		
20	142	1	127		122		95			
21	168	1	128	1	106		125			
22	156		120		112		120			
23	158	1	120		116		111			
24	148		140		117		108			
25	138		138		118		106			
26	148		140		114		110			
27	147	1	135	1	123		112	1		
28	138		115		128	1	103	1		
29	147	2	145		138		120			
30	145		125	1	128	1	115			
31	137	1	135		110		118			
32	127		125		120	1	110			
33	137		130		130	1	115			
34	135	1	122		120	3	110	1		
35	133		132		117		108			
36	138		150	2	130		120			
37	122		132	1	138		116			
38	138	1	128		136	1	110			
39	132	1	144	2	122		113			
40	134	1			127		113			
41	130				133		110			
42	140				135		135			
43	130				127		120			
44	138				127		107			
45	130	1			122		125			

tion in the form of the work curve for any individual from day to day.[1] Among the daily records can be found some which

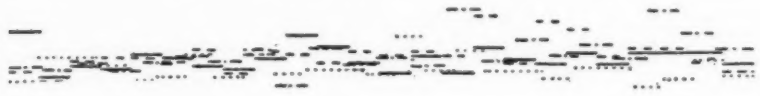


FIG. 1. Work Curves of Subject *D*. Four Days.

FIG. 2. Work Curves of Subject *L*. Four Days.

FIG. 3. Work Curves of Subject *Mc*. Four Days.

[1] The variation from day to day in the form of the curve of work may, in the case of one subject, *R*, be due in some considerable degree to the use of a too coarse unit in recording the time. But such is not the case with any other subject.

could be used, if taken singly, as excellent examples of initial spurt, end spurt, warming up, or fatigue, of various types. But such selection of a single day's record is obviously unjustifiable, since the same person on another day shows opposite fluctua-

FIG. 4. Work Curves of Subject *R.* Four Days.

FIG. 5. Work Curves of Subject *S.* Four Days.

In Figs. 1–5 the long dashes all refer to the 3d day's work curve; the short dashes, to the 2d day's; the dotted lines, to the 4th day's; and the dash and dot lines, to the 1st day's.

tions.[1] Many different days' records are necessary to determine the forms of an individual's work curve.

[1] The inconstancy of the same individual in respect to the form of the work curve would not have been so great if the work had been that of saying to oneself or writing down the results of *each successive* addition, or if the subject had not expected a heavy penalty for an error, or if the subject had not pushed himself to the utmost, or if the subject had had a very great deal of special practice in adding at his maximal rate, but without trying to add by grouping. But in any case the variation in the form of the work curve on different days is so great as to require careful consideration of the unreliability of any determination based on only a few days' records. These unreliabilities have rarely been considered by Kraepelin and his followers, so that it is possible that the fluctuations to explain which they have invoked initial spurt, final spurt, spurt after fatigue, spurt after disturbance, or the rhythm of attention are one and all accidents of the unreliability of the determinations.

Initial spurt, if a real fact, will be found in an examination of the work, minute by minute, of the first quarter of an hour. Kraepelin supposes it to be a phenomenon chiefly of the first five minutes. It is possible to find two or three single records which might, if alone, be so interpreted, the most plausible case being one of subject *L*'s: 265 seconds for the first 16 examples (no errors), 335 seconds for the next 16 (2 errors), and 360 seconds (4 errors) for the next 16. But these single instances are demonstrably not due to any consistent initial spurt. Exactly opposite instances occur, and in the same individuals. The average time (plus 5 seconds for each error) for each subject, for the four days' trials of each successive row is given in Table II., including for each individual approximately his first twenty minutes' work in each of the four work periods.

TABLE II

Average time (corrected for errors) required to add successive rows of 16 ten-digit examples at the beginning of the four work periods. Five subjects—*B*, *L*, *Mc*, *R*, and *S*. Time in seconds.

Row	Subject				
	D	*L*	*Mc*	*R*	*S*
1	100	284	164	186	149
2	100	311	146	183	142
3	104	318	143	206	155
4	100	301	138	204	158
5	99		137	186	147
6	95		126	195	146
7	91		120		159
8	90		128		
9	113		136		
10	105				
11	96				
12	99				

Each 'row' equals 16 examples or 144 additions, involving the writing of 16 two-place numbers as answers, and one observation of the watch and record of the time-

The absence of any evidence of initial spurt in the facts of Table II. is better realized by presenting them graphically. Let each centimeter horizontally approximate five minutes, let the same vertical height represent approximately the average efficiency of the function for each individual. We then have approximately Fig. 6, which shows the absence of any uniform fluctuation of any sort.

Or we may go back to the original scores and compute the work for each successive five minutes for each individual. The results are as in Tables III. and IV. Weighting the results

FIG. 6. Average Work Curves of the First 20 Minutes. Each Sort of Line (Dot, Short Dash, Longer Dash, etc.) Represents the Average Work Curve of One Subject.

FIG. 7. Average Work Curves of the Last 20 Minutes. Each Sort of Line (Dot, Short Dash, Longer Dash, etc.) Represents the Average Work Curve of One Subject.

from all five subjects equally and averaging the five, we have the efficiencies for the four successive periods standing in the following proportions: 100, 99, 101 and 100.

TABLE III

Average number of examples done (each involving 9 additions, and the writing of a two-place number[1]) and of errors made, in each five-minute period of the first 20 minutes of work.

	D		*L*		*Mc*		*R*		*S*	
	Ex.	Er.	Ex.	Er.	Ex.	Er.	Ex.	Er.	Ex.	Er.
First 5 min...	48.8	1.7	16.8	1.5	31.3	.7	26.3	.7	33.6	1.4
Second 5 min.	49.9	1.0	15.8	1.7	34.8	.8	24.5	1.2	31.6	1.5
Third 5 min..	48.2	1.0	15.6	2.0	37.8	.7	25.5	1.9	33.5	1.7
Fourth 5 min.	48.6	.8	16.2	.7	37.4	1.1	24.9	1.9	31.5	2.3

[1] Because of the time required to write the answers and to record the time after every 16 examples, the number of additions that would have been made in mere addition may be taken to be not 9 but 10 times the numbers entered in the table.

TABLE IV

Per cent. which the efficiency in each successive five-minute period of the first 20 minutes of work is of the individual's average efficiency for the 20 minutes.

	D	*L*	*Mc*	*R*	*S*
First 5 min.....	100	104	89	104	103
Second 5 min.....	102	98	99	97	97
Third 5 min.....	98	97	107	101	103
Fourth 5 min.....	99	100	106	98	97

I am unable to find anywhere any evidence of consistent initial spurt with any individual in all mental functions or with all (or nearly all) individuals in any kind of mental work, much less with all individuals in all kinds of work. The work curves obtained by Oehrn,[1] Amberg,[2] Weygandt,[3] Lindley,[4] and other workers in Kraepelin's laboratory give no such evidence. Nor is it to be found in the data got by Yoakum,[5] so far as he presents them.

Lindley's work, which was the most extensive, showed as speed ratios for successive five-minute periods at the beginning of work, 100, 98, 97, 97, and 96, using the data from all three subjects. The first five minutes, that is, differed from the second substantially only as the fourth from the fifth. Putting together Weygandt's series I find ratios of 100, 97 and 95½ for the first three five-minute periods. Hoch showed, on the whole, ratios of 100, 99, 98 and 94 for the first four five-minute periods. Miesemer[6] showed ratios of 100, 96, 98, 97. In fact the very results of Rivers and Kraepelin,[7] to explain which Initial Spurt was specially invoked, give as ratios (by five-minute periods) 100, 87, 99, 101, 102, 102. Clearly the fact in them to be

[1] Oehrn, A., '95, 'Experimentelle Studien zur Individualpsychologie,' *Psychologische Arbeiten*, Vol. I., pp. 92–151.

[2] Amberg, E., '95, 'Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit,' *Psychologische Arbeiten*, Vol. I., pp. 300–377.

[3] Weygandt, W., '97, 'Ueber den Einfluss des Arbeitswechsels auf fortlaufende geistige Arbeit,' *Psychologische Arbeiten*, Vol. II., pp. 118–202.

[4] Lindley, E. H., '00, 'Ueber Arbeit und Ruhe,' *Psychologische Arbeiten*, Vol. III., pp. 482–534.

[5] Yoakum, C. S., '09, 'An Experimental Study of Fatigue,' Psychological Monograph No. 46 of the PSYCHOLOGICAL REVIEW.

[6] Miesemer, K., '02, 'Ueber psychische Wirkungen Körperlichen und geistiger Arbeit,' *Psychologische Arbeiten*, Vol. IV., pp. 375–434.

[7] Rivers, W. H. R., and Kraepelin, E., '96, 'Ueber Ermüdung und Erholung,' *Psychologische Arbeiten*, Vol. I., pp. 627–678.

explained is the 87 of the second five minutes rather than the 100 of the first five.

It is, I admit, very likely that some individuals in some kinds of work tend to fall off rapidly from the too exacting standard which they set themselves at the beginning, just as some tend to rise rapidly above the standard which they cautiously try at the beginning. But these idiosyncrasies must not be misinterpreted as a general law. Until some hypothesis can be framed about initial fluctuations which will enable one to *prophesy* the form of the work curve, if only for a single individual or for a single function, these idiosyncrasies belong under the heading of 'accidental' or 'chance' variations—that is, of fluctuations whose causes are unknown.

End Spurt

It is often the case in ordinary mental work with a time limit, that as one approaches the end of the work period, the knowledge that he is approaching it leads him to spurt. In ordinary mental work one does not work throughout at one's possible maximum, so that such a spurt is easily possible. In experimental work, when the subject is required to work throughout at maximum possible efficiency, such a spurt can come only if the subject has deliberately disregarded instructions, or if the knowledge of the approaching end releases forces over which he had no control. The latter is apparently possible, various external stimuli, such as other competing individuals, applause and the like, being apparently able to add a reinforcement beyond what a subject's own determination can summon.

How common an occurrence such a final spurt is in work carried on with no deliberate reservation of power, and of how great a magnitude it is, may be estimated[1] from the following facts:

It is not common or important enough to make the last eighth of a two-hour period one per cent. more efficient than

[1] For a demonstration of the existence and amount of end-spurt one should have subjects work to the end of a given period with and without knowledge that the end was approaching, all other conditions being identical.

the average eighth in the ten individuals each measured in six sorts of work by Oehrn.

In 16 subjects working for very long periods (4–12 hours) the effect of end-spurt in those who worked to a time limit is not great enough to counterbalance the effect (real or possible) of the decision of some to stop work because of having made a series of very inferior records.

In the four two-hour tests in addition previously described, not one subject of the five showed a *consistent* tendency to increase his efficiency by even five per cent. during the last 5 or the last 10 minutes. For the five subjects whose records have been computed in detail the facts are given in Table V. and Fig. 7. (In Fig. 7 as in Fig. 6, each centimeter

TABLE V*a*

Average time (corrected for errors) required to add successive rows of 16 ten-digit examples at the ends of the four work periods. Five subjects—*D*, *L*, *Mc*, *R*, and *S*. Time in seconds.

	D	*L*	*Mc*	*R*	*S*
Eleventh from last row added	105				
Tenth from last row added...	104				
Ninth from last row added...	104				
Eighth from last row added...	108		128		130
Seventh from last row added..	113		130		134
Sixth from last row added....	103		126		148
Fifth from last row added....	110		125	205	142
Fourth from last row added...	108		126	205	141
Third from last row added....	110	297	143	203	137
Second from last row added..	101	254	129	188	138
Next to last row added.....	104	365	130	190	110
Last row added.....	95	278	134	200	126

TABLE V*b*

Average time (corrected for errors) required to add successive equal amounts (3 rows for *D*, 1 row for *L*, 2 rows for *Mc*, 1 row for *R*, 2 rows for *S*) at the ends of the four work periods.

	D	*L*	*Mc*	*R*	*S*
Fifth from last.....				205	
Fourth from last.....				205	
Third from last.....	313	297	256	203	282
Second from last.....	324	254	251	188	283
Next to last.....	328	365	272	190	275
Last.....	300	278	264	200	236

horizontally approximates 5 minutes; the same vertical height is used to represent approximately the average efficiency of

the function for each individual.) There is a probable tendency to fluctuation in the direction of 5 per cent. greater efficiency in the last few minutes, but the central tendency is almost certainly not to a change of over + 10 per cent., and is perhaps to as little change as 0.

Amberg's average results at the end of one- or two-hour periods of adding were, by five-minute periods, in the ratios 100, 101, 101 and 96. Weygandt's average results in the last twenty minutes of 90 minutes' continuous adding were in the following ratios:

Subject A	102	95	100	107
Subject B	100	99	99	100
Subject D	100	99	102	98
Average	101	98	100	102

Of Lindley's three subjects one did a bit more, one a bit less, and one almost exactly the same amount of addition per minute in the last 5 as in the average of the last 20 minutes. Combining the data for all three there is not a difference of one per cent. Miesemer's results for the last four five-minute periods of an hour's adding were in the proportions 101, 100, 99, 106.

On the whole, no subject who has been tested four or more times shows consistently any considerable end-spurt and the general tendency is to a rise, in the last five or ten minutes, of not over three per cent.

Spurt after Fatigue and Spurt after Disturbance

In mental work in ordinary life a person may obviously, if he is not doing his best, at any time do a little better to make up for an observed temporary deficiency however caused. Deficiencies due to disturbances certainly, and to fatigue, if that acted unevenly throughout a work period, might be thus noticed and counterbalanced. In a subject who is keeping his efficiency at a maximum so far as he can control it, the observation of a fall in achievement might still so act as a reinforcement. Kraepelin and his followers assume that it does, and seem to regard each 'drop-rise' sequence in the

curve of work as a deficiency caused by fatigue or disturbance which stimulates a gain in efficiency as a result of an Ermüdungsantrieb or Störungsantrieb.

It should be noted, however, that on general grounds the suggestion that one is doing well would seem more favorable to the efficiency of one already doing his best than the suggestion that he is doing badly, and that empirically no one has correlated the fluctuations in work curves with the incidence of disturbances of known character. The doctrine of spurt after fatigue and spurt after disturbance in the case of work done under the conditions of the ordinary fatigue experiment, is then at present a speculative hypothesis. It was devised apparently to explain the fluctuations in efficiency, from one minute or five-minute period to another, which are found in continuous adding, cancelling letters, memorizing, or other forms of mental work.

A rise following a fall in the curve easily attracts observation and tempts readily to theorizing. A rise followed by a rise or a fall followed by a fall is not so striking. The explanation of a 'fall-rise' sequence by spurt after disturbance or spurt after fatigue is really unwise, however. For if the fall is caused by a disturbance, no cause is required for the rise save the ending of the disturbance; while if no external cause is known for a given fall, there is no reason why one should pretend to know the cause of its sequent rise. The wiser effort would be to seek hypotheses which would account for rise-fall, rise-rise, fall-fall, and fall-rise sequences, one and all, and, until such hypotheses could be subjected to verification, to be content with attributing them to 'accidental' variations.

Rhythm of Attention

I have no new data to report bearing on the theory of Voss[1] that efficiency in adding fluctuates in a rhythm of 2 up to 3 seconds. But since Voss's work has been referred to approvingly by later writers, it may be worth while to show its essential unsoundness.

[1] von Voss, G., '99, 'Ueber die Schwankungen der geistigen Arbeitsleistung,' *Psychologische Arbeiten*, Vol. II., pp. 399–449.

Voss's study ('99) is instructive as a sample of the difficulty of keeping in mind just what a set of measurements means while one is arguing from them. What he does is in essence as follows:

Suppose the times of a series of additions are, in order, in fifths of a second, 3–3–6–5–3–8–3–1–3–1–3–4–3–3–3–3–6–2–3–3–5–4–5–3–3–2–4–2–3–3–4–2–7–3–3.

What Voss calls the duration length of a fluctuation is such a time as, in this case: 3 plus 3 plus 6; or 5 plus 3 plus 8; or 3 plus 1 plus 3 plus 1 plus 3 plus 4; or 3 plus 3 plus 3 plus 3 plus 6; or 2 plus 3 plus 3 plus 5; or 4 plus 5; or 3 plus 3 plus 2 plus 4; or 2 plus 3 plus 3 plus 4; or 2 plus 7. That is, Voss divides the entire work into a series of lengths of time called fluctuations—in this case 12, 16, 15, 18, 13, 9, 12, 12, 9—each ending with an addition slower than any since the end of the last fluctuation. The length of each fluctuation is then always the sum of *two or more* single addition times.

Now, this being the method of scoring and the time of three fifths of a second being the most common, differences of three fifths of a second in the fluctuations *must* be the most common differences. The case is as if one reckoned fluctuations in the income of a man whose salary was amost always raised or lowered by approximately $100.00. The fluctuations would of course almost always vary by steps of approximately $100.00.

Voss, finding such a variation, hails it as important and discusses seriously explanations of this, to him, very remarkable phenomenon, by attention waves and other features of the worker, but finally observes that it is due to the fact that by the artificial conditions of the test and method of scoring one fluctuation must differ from another in length by the length of one, two or more additions.

He clings, however, to the faith that the great frequency of fluctuation lengths from 10 to 13 fifths of a second has some real and unitary cause in the constitution of the worker. He considers the relation to the respiration curve, and to the optimum time between the warning signal and the stimulus in experiments in reaction time, and concludes that the great frequency of fluctuations of 10 to 13 fifths of a second is due

to the fact that 'the attention tends to rise to its highest intensity (*Spannung*) in periods of somewhat over 2 seconds,' so that 'the most frequent length of a fluctuation expresses exactly the length of fluctuations of attention found in other investigations!'

There are two flaws in this argument. First, the length of fluctuation which Voss measured is not the length from a point of maximum efficiency to the next maximum nor from a minimum to the next minimum. Such lengths can be found only by somehow defining 'maximum' (say as an addition of 2/5 seconds or under) and 'minimum' (say as a time of over 5/5 seconds), and by them measuring the intervals. Voss's measures are of the intervals between *all* turning points. Such sequences as 1–2–1, 2–3–2, 6–7–6, or 7–8–7 give end-points for his fluctuations as truly as do the sequences 1–3–5, 4–1–2–6 and 3–1–1–5 in the series 6–1–3–5–4–1–2–6–3–1–1–5.

In the second place, the variation of single addition-times being around such modes as he indicates with such frequencies as he indicates, their appearance in an order *absolutely random* as respects speed, would give substantially as great a preponderance of fluctuations of 10–13 fifths of a second as he found. It seems to have been a misunderstanding of the laws of probability that led Voss to think that such a random order would make fluctuations of 7 fifths of a second the most frequent. It certainly would not.

Since Voss does not give the required data, I cannot calculate exactly what the relative frequencies of different lengths of fluctuation would be by mere chance, but they would certainly show a clustering closely corresponding to that which he explained by the rhythm of attention. I take at random his experiment VIII. Suppose the time for the separate additions to be such that for every addition taking 9 fifths of a second there occurred one taking 8 fifths of a second, two taking 7, three taking 6, six taking 5, eleven taking 4, 73 taking 3, and three taking 2. This is very close to the distribution in Voss's experiment, given on page 409 of his report. Suppose the order of occurrence of these times to be absolutely random. Then about a third of all the 'fluctuations' as defined by Voss

would be from 10 to 15 fifths of a second inclusive. This is the fact which he finds (on page 425 of his report) and from which he reasons to a 2–3 second rhythm with the attention-wave as a cause! The same will be found to hold for all the results of the experiments reported by this author.

Warming Up

The best definition of 'warming-up' as an objective act is that part of an increase of efficiency during the first 20 minutes (or some other assigned early portion) of a work period. which is abolished by a moderate rest, say of 60 minutes. Such warming up should show itself clearly in individuals at or near the limit of practice, and, in others, should compound with the effect of practice to make the rise in efficiency especially rapid in the first twenty minutes of work, or the fall (supposing the function to diminish in efficiency) specially slow in this same period. What time is assigned in the definition of warming-up effect is of little consequence to the investigation so long as *some* time is assigned.

In the case of my five subjects in addition it is clear from Figs. 1–5 that such objective warming-up either vanished within very few minutes or was so slight as not to appear consistently and not to appear at all in any considerable magnitude.

There is little or no direct evidence of warming-up in the records got by Oehrn, Lindley, Weygandt, Bolton,[1] Miesemer, or Rivers and Kraepelin. My sixteen subjects working at mental multiplication of a three-place by a three-place number showed signs of its presence, but not conclusively.

Since Oehrn's results are important for the consideration of both warming-up and fatigue, and since they were, like those of my five subjects, for two-hour periods, I give later a brief graphic summary of them. It will be seen from Figs. 8, 9 and 10 that, if a slight allowance is made for the greater effect of early than of late practice, no special warming-up effect is needed to account for the very slightly less decrease in efficiency during the first twenty minutes than during the average twenty minutes.

[1] Bolton, T. L., '02. 'Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut und Muskelleistung,' *Psychologische Arbeiten*, Vol. IV., pp. 175–234.

It seems certain, however, from the cruder observations of daily life, that for many individuals in many functions, there is a warming-up effect as defined, but I am unable, with the data at hand from Kraepelin's pupils or others, to separate out this temporary improvement that comes at the beginning of the exercise of a function after rest, from the more permanent improvement that comes from exercise of the function in general. Experiments near the limit of practice are necessary. One must be careful not to mistake the sudden gain from practice in its early stages for warming-up. There is a temptation, for example, to see evidence of the latter in the first day's curves for my five subjects (shown in Figs. 1–5). But the absence of any similar gain in the first half hour in the curves for the last day for these same subjects proves that its presence on the first day was probably a result of practice.

It should also be noted that intellectual warming-up in the popular sense refers rather to fore-exercise of *other functions*, in order to get materials and motives with which and by which the given function is to work, than to an intrinsic alteration of it. Such is the case, for example, with 'poets, artists and composers' of whom Mosso[1] speaks.

Adaptation (Gewöhnung)

Adaptation is, so far as I can see, definable objectively only as a slower, longer and somewhat more permanent warming-up effect. I am unable to identify or measure it in the accessible work-curves.

Fatigue

Fatigue can be best defined objectively as that diminution in efficiency *which rest can cure*. The fatigue caused by a given amount of work without rest is then measurable by the difference between the efficiency at the end of that work period and that found after a period of rest plus, if necessary, a period of warming up. Since, in the case of addition of ten-digit columns, warming-up is approximately zero, fatigue may be measured by the rise from the end of a work period to the beginning of work after rest.

[1] Fatigue, Eng. translation of '04, p. 299.

The advantage of so defining the fatigue effect is that its mixture with practice effect is obviated. If the more usual definition—as the decrease in efficiency caused by the exercise of the function without rest—is used, we are compelled, unless an individual is already at the limit of practice, to analyze (by indirect means) the total change with continuous exercise into a difference between a supposed increased efficiency due to the exercise and a supposed decreased efficiency due to its continuity. The best method of approximately attaining this end is precisely to compare the efficiency at the end of one work period with the efficiency at the beginning of the next after adequate rest (or rest plus warming-up). So it is wiser to define fatigue as that fact from which in any case we infer, and by which we measure, it.

In the case of the five subjects the difference in efficiency between the last ten minutes of 1½ or 2 hours adding and the first ten minutes of the next period, was as shown in Table VI. The central tendency is toward a fatigue effect of 6 per cent. (*i. e.*, to do 6 per cent. fewer examples in the same time) from approximately 100 minutes addition.

TABLE VI

Time (corrected for errors) required to add x rows at the end of each work period and at the beginning of the following work period. $x = 6$ for *D*, 2 for *L*, 4 for *Mc*, 3 for *R* and 6 for *S*. Also the per cent. which the latter is of the former in each case.

	1*c*. At End of Work Period 1	2*b*. At Beginning of Work Period 2	2*c*. At End of Work Period 2	3*b*. At Beginning of Work Period 3	3*c*. At End of Work Period 3	4*b*. At Beginning of Work Period 4	2*b*/1*c*	3*b*/2*c*	4*b*/3*c*	Av. *b*/Av. *c*
D	715	565	595	620	615	580	79	104	94	92
L	975	615	590	560	545	585	63	95	107	88
Mc	543	531	579	510	511	466	98	88	91	92
R	630	545	615	545	535	495	87	88	93	89
S	897	850	842	934	779	743	95	111	95	100

Average of all 15 per cents = 93. Median of all 15 per cents = 94.

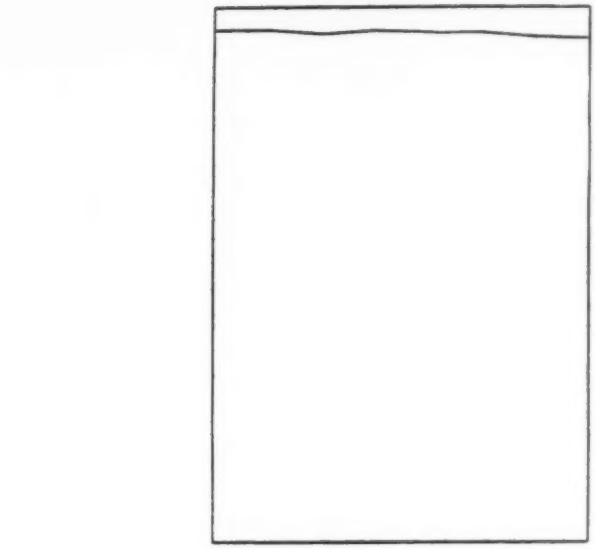
To those who have accepted certain common notions about mental fatigue this may appear preposterously small and at variance with the results obtained by other workers. This appearance is due in the main to a confusion of the conclusions

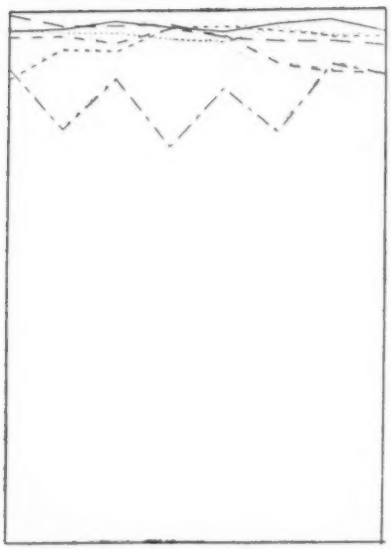
of these other workers with their data. The actual facts concerning the diminution of efficiency in a function as a result of its exercise for two hours without rest show in general an effect so slight as to be almost or quite balanced by the practice effect of the exercise.

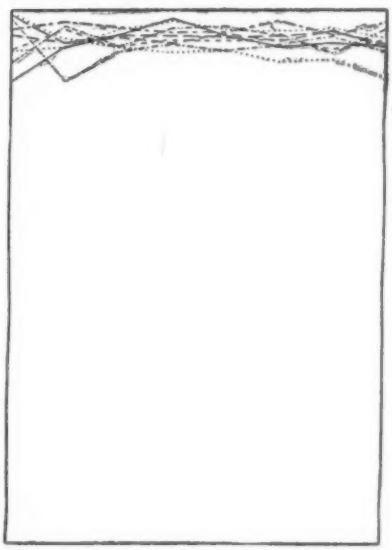
Indeed, the most extensive study of Kraepelin's own pupils, that of Oehrn, gives as a general result from 60 two-hour work periods (ten subjects, each with six sorts of work) a curve for the eight successive 15 minute periods as shown in Fig. 8. In general, that is, the subjects worked equally well throughout the entire two hours. This general result might have come from a steady improvement in some of the functions, balanced by a steady loss in efficiency in others, or from various rates of change in efficiency in the different functions. As a matter of fact, however, as Fig. 9 shows, the different particular functions follow closely the general tendency. Their slight divergences therefrom are probably due to the small number of subjects and experiments. Each person, too, might have had a different work-curve, some getting better at points when others were getting worse. As a matter of fact, however, the ten individuals' work-curves follow closely the general work-curve which represents the central tendency of them all. The facts are shown in Fig. 10. Lindley's three subjects gave as a median result for continuous adding for one hour the curve shown in Fig. 11.

In general, in the case of the mental functions so far tested, the subjects maintain about the same efficiency from the beginning to the end of a work period of one or two hours unless the function is one at which they have had little practice. Then they usually *improve* from beginning to end. It is a fact, though one overlooked by many writers on fatigue, that a diminution in efficiency of over ten per cent. from two hours exercise of a function has very rarely been observed directly.[1]

[1] The two clear cases of a large fatigue effect are the results of Weygandt and Bolton. I calculate from Weygandt's data that with his three subjects 24 hours rest increased the number of additions done in 15 minutes by a fourth over the number done in the last 15 minutes of a 90 minute work period. From the four work periods that are available in Bolton's data it appears that 23½ hours rest plus a half hours' practice increased the number of additions by three fifths over the number done in the last 15 minutes of a two hour work period; and that a half hour's rest made an increase of a fourth. Such cases are rare.







FIGS. 8, 9, AND 10. The Results of Oehrn's Measurements of the Efficiency of Eight Successive 15-Minute Periods in the Case of Ten Subjects, Each Tested in Six Different Functions. Fig. 8 shows the Central Tendency of All Sixty Work Curves. Fig. 9 Shows the Work Curves in the Cases of the Six Functions Tested. Fig. 10 Shows the Work Curves in the Case of the Ten Subjects.

Such has been inferred, either as a result of false interpretations of the units of measure, or of the supposition that any decrease in two hours continuous work below the practice effect when the two hours are distributed in the best possible manner is to be reckoned as fatigue.

The latter procedure seems plausible, but is essentially unsound. For, example, suppose that I had argued concerning

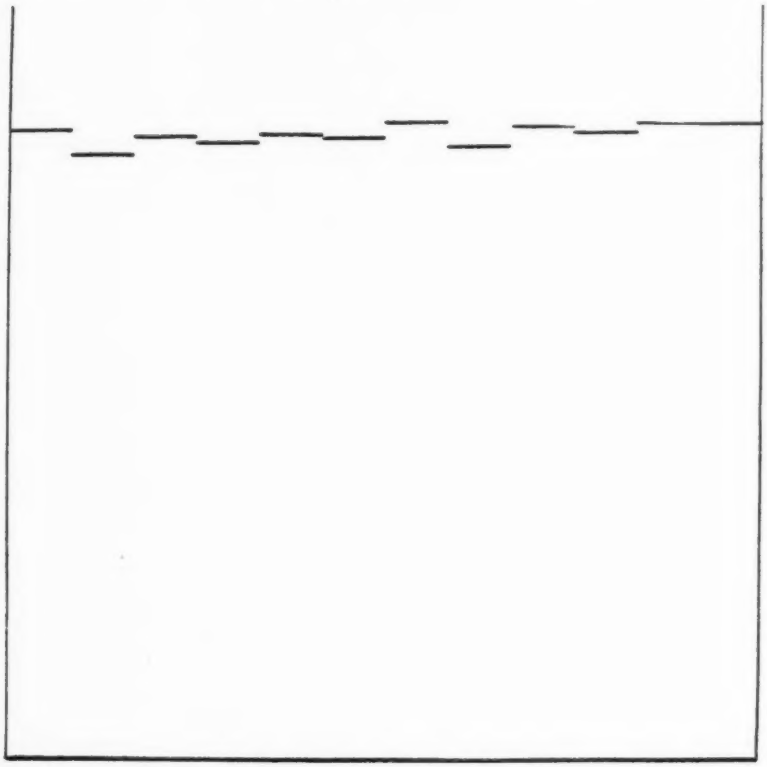


FIG. 11. The Average Results of Lindley's Measurements of the Efficiency of Twelve Successive 5-Minute Periods in the Case of Three Subjects.

my five subjects as follows: An hour of exercise in adding 10-digit examples distributed in seven daily work periods of approximately equal length produces in educated adults an improvement in the time (corrected for errors) of 30 per cent.[1]

[1] This has been found to be substantially correct by Thorndike and by Wells.

Two hours of exercise distributed over 14 days may fairly be expected to produce an improvement of at least 38 per cent. Two hours of continuous practice, however, produced in our five educated adults a change of less than 8 per cent. improvement. Therefore the fatigue due to the two hours work is at least 30 per cent.

Every one of these statements is true except the last. Its falsity is revealed by the following facts: First, on the next day at the beginning of work these subjects did not do 30 per cent. better. They did as a matter of fact 12 per cent. better. Second, if the experiment is continued, and fatigue is again estimated by this method of comparison of actual gain or loss with the gain from equal exercise optimally distributed, the amount of fatigue decreases enormously. No one acquainted with the facts would claim for the last two hours of eight optimally distributed, a practice gain in educated adults of over 13 per cent.[1] It would probably be very, very much less. But in the two continuous hours forming the 7th and 8th in the experiment, the general change was a loss of 1 per cent. The fatiguing effect of this two hours is thus, at the most, 14 per cent., or half of what it was a few days earlier. This discrepancy has to be explained by supposing that the four two-hour periods of exercise have had a three-fold effect: (1) that they have permanently improved ability as much as eight hours optimally distributed exercise would have done, but (2) they have also temporarily so diminished ability as to counterbalance this on each day and (3) they have also enormously diminished their power to temporarily diminish ability. Unless the first and third of these statements are true the second cannot be. The first we have seen to be false in the case of the two hours exercise.

But it must *always* be false unless the optimal distribution of time is that of the work period of the fatigue experiment in question. For one distribution of time, D_1, to give the same amount of permanent improvement as another, D_2, can mean only that in later trials under identical conditions those who

[1] In the study of one hour's distributed exercise quoted above, nearly three fourths of the gain was made in the first half of the exercise.

had been subjected to D_1 show the same superiority to their earlier scores as do those who have been subjected to D_2.

In general, if two random groups, A and B, of equal ability at the date D_0 in the function M, are both given X hours practice during the time from D_0 to D_n, and if in a selection of tests after D_n that are random as to date, length and all other external conditions, groups A and B show equal efficiency, then, no matter what difference there was between A and B in the way the X hours were distributed, that difference can have made no difference in the amount of permanent improvement in M.

The test of the relative practice effect of continuous and broken exercise is not in some mythical changes which would show themselves if fatigue, warming up, additional practice and the like did not interfere and hide them. It is in a fair sampling of some actual operations of the function.

In short, permanent improvement can mean only the gains shown, first, at the end of work plus a length of time such as just provides full rest, and second, at any given selection of later dates. If continuous work does produce the same permanent improvement as optimally distributed work, then the decrease of the score at the end of a two-hour period below that at the beginning of the next is the measure of its smaller temporary improvement. If continuous work does not produce the same permanent improvement as optimally distributed work, the measure of its smaller temporary improvement cannot be the decrease in the gain from X hours' continuous work below what would have appeared if the X hours had been optimally distributed.

Practice

If fatigue is defined as the diminution in efficiency which rest can cure, practice effect may be objectively defined as the increase in efficiency due to a given amount of exercise distributed in a given manner, the conditions of rest and fore-exercise being the same. That is, the practice effect is always a measure of a difference between two tests separated by an interval of time. It is the effect of that interval as well as of

the exercise which fills all or part of it. However practice effect be defined, the fact from which it is in any case inferred is always X exercise of the function distributed in some way over Y time. In once for all accepting this complex causation of the practice effect, we really simplify thought.

Kraepelin, in assuming that the practice effect proper should be defined as the increase in efficiency due to the *mere amount* of exercise of the function, has to assume further an effect of the *mere time*, whereby a constant loss of practice effect is taking place. Thus any actual measure of improvement from one test to another is the result of $GP-LP$, the gain due to the practice alone less the loss due to the time alone. The inference of the amounts of GP and LP permits speculative fancy to run riot. An innocent improvement of 2 per cent. from the first quarter hour's work of an hour on Monday to the first quarter hour's work of an hour on Tuesday may thus become a gain of 10 per cent. from the practice minus a loss of 8 per cent. from 24 hours, or a gain of 20 per cent. minus a loss of 18, or a gain of 200 per cent. minus a loss of 198! We have also the scientific fiction of a function gaining and also losing during the very hour of its exercise—gaining by the exercise and losing by the hour.

The naïve procedure of leaving the 'amount of exercise distributed in a given way' as the unanalyzed cause of the practice effect, which has been customary amongst psychologists other than Kraepelin and his followers, seems therefore preferable.

Summary

The essential empirical facts about the curve of mental work seem then to be as follows: Two hours or less of continuous exercise of a function at maximum efficiency, so far as the worker can make it so, produce a temporary negative effect, curable by rest, of not over ten per cent., and in most functions still less than that. The permanent practice effect is much less than that of equal time distributed in fractions over a week or more. Fluctuations of considerable amount occur in any one work period for any one subject, but except for a rise in efficiency of approximately 4 per cent. near the end when the

date of the end is known, no regularity in them has been proved for any one of them for any one subject in any one sort of work, much less for any one subject in all sorts of work, or for all subjects in any one sort of work. The supposed laws that the very first few minutes and the minutes after a drop in efficiency are periods of specially high efficiency are not supported by the facts. A special gradual increase in efficiency in the first fifteen or twenty minutes is not demonstrable in the case of the simple functions such as addition, mental multiplication, marking words of certain sorts and the like. The fluctuations in a single day's record for a single subject are then in no sense explained by referring them to fervor at starting, fervor after disturbance, fervor after fatigue, incitement or adaptation.

The most important fact about the curve of efficiency of a function under two hours or less continuous maximal exercise is that it is, when freed from daily eccentricities, so near a straight line and so near a horizontal line. The work grows much less satisfying or much more unbearable, but not much less effective. The commonest instinctive response to the intolerability of mental work is to stop it altogether. When, as under the conditions of the experiments, this response is not allowed, habit leads us to continue work at our standard of speed and accuracy. Such falling off from this standard as does occur is, in the writer's opinion, due to an unconscious reduction of the intolerability, by intermitting the work or some parts of it.

By this view we have a possible explanation of the decreased practice- or learning-value from continuous rather than distributed exercise. The connections, in the former case, though formed as frequently, would result in far less satisfaction and so in less close bonds. There are, of course, other advantages in distributed exercise, but this difference in satisfyingness would explain the notable puzzle that the permanent effect of work without rest upon learning is greater than its temporary effect upon efficiency.

Speculative Analyses of the Curve of Work

So far in this paper fatigue, warming-up, initial spurt and the rest have been considered as objective features in a work-curve. Psychologists have also often used these terms as names for mysterious forces which caused efficiency to rise and fall. *Antrieb* is thus an inner fervor or impulse which causes a spurt; *Anregung* is thus an inner incitement which causes the warming-up effect. *Fatigue* is thus the inner cause of diminished efficiency rather than the diminution itself.

A work-curve may then be considered as the gross result of a compounding of all these forces in various degrees, and may be analyzed into imaginary curves, each corresponding to the action of one of these forces. The analysis which anyone makes will depend upon his theories about their separate action, and will display those theories clearly.

Let us then, for contrast with the views which I have suggested in this paper, examine Kraepelin's analysis of the following work-curve: In six successive 5-minute periods before a 30-minute rest, and in six immediately thereafter, the observed numbers of additions were: 483, 473, 478, 473, 486, 474, rest, 496, 510, 513, 489, 497, 494.

Kraepelin's explanation of the above case is[1] as follows: The efficiency of the function, apart from practice, fatigue, incitement, adaptation, and fervor from starting, finishing, etc., is 393. The effect of practice is to raise this by 56, 40, 37, 34, and 32 from one to another of the first six periods respectively; of this total practice gain of 199, 149 is lost during the 30 minutes of rest;[2] during the last half hour the practice gains from one to another of successive 5-minute periods are 31, 29, 27, 26, 25. Thus work under the influence of only practice and the 30-minute rest would, in Kraepelin's opinion, give a curve of 393, 449, 489, 526, 560, 592 (rest here), 443, 474, 503, 530, 556, 581 as shown in the upper continuous line of Fig. 12.

The effect of fatigue is to lower efficiency from period to

[1] Kraepelin, E., '02, 'Die Arbeitscurve,' *Philosophische Studien*, Vol. 19, pp. 459–507.

[2] Or strictly from the last 5-minute period before to the first one after the rest.

period successively by 41, 41, 41, 41, 41, a total loss of 205; the rest raises it by 197 (in divisions per 5 minutes of 80, 45, 35, 26, 7, 2, 2); after rest fatigue again lowers it in successive steps by 41, 41, 40, 39, 38. The work under the influence of only fatigue and the rest would, in his opinion, give a curve of 393, 352, 311, 270, 229, 188, (rest here) 385, 344, 303, 263, 224, 186, as shown in the dash line of Fig. 12. Practice, fatigue and the rest together would give the middle curve of Fig. 12.

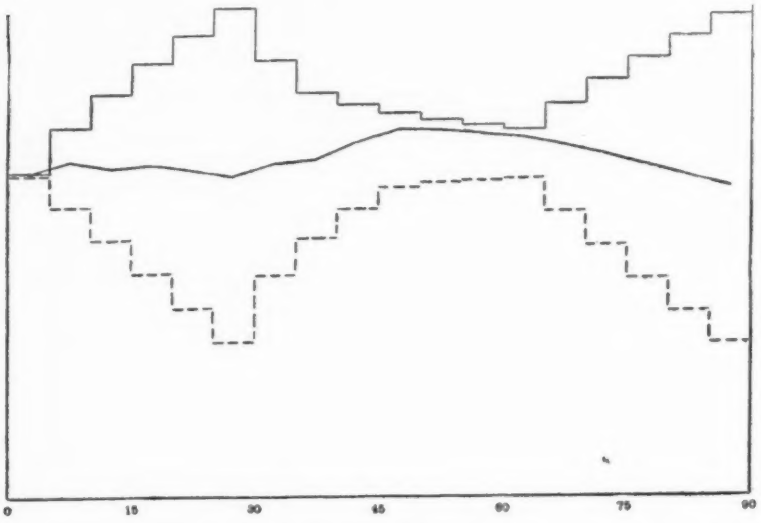


FIG. 12. Kraepelin's Estimates of the Effects of Practice and Fatigue during 30 Minutes Continuous Adding, 30 Minutes Rest, and 30 Minutes Continuous Adding. The Horizontal Scale is of Time; the Vertical Scale is of Number of Additions. The Upper Line gives the Efficiency of the Function by Practice; the Lower Line Gives the Efficiency of the Function by Fatigue; the Middle Line Gives the Combined Result of Both.

Incitement or 'warming-up' is supposed by Kraepelin to raise efficiency from period to period by 35, 5, 5, 0, 0, all the 45 being lost during rest and the gains after rest being 35, 10, 0, 0, 0. The effect of adaptation is supposed to be, from period to period, 30, 5, 5, 5, 5; 5 of this total 50 is lost during rest; after rest the gains due to it are 5, 4, −8, 10, 1. This effect of incitement is shown in Fig. 13 by the line of long dashes; that of adaptation by the line of short dashes.

The effect of fervor at starting is to make the first five-minute period better by 90, and the first one after rest better by 16, than they would otherwise have been.

Minor fluctuations make the third period 4 worse, the fourth, 15 worse, and the sixth, 8 worse, than they would otherwise have been. The ninth and tenth periods are so influenced to the extent of − 1 and − 2. Fervor at the approach of the end makes the last period of the whole series better by 18, and the next to the last better by 9, than they would otherwise have been. The effect of these various *Antriebe* is shown by the dot and dash line of Fig. 13.

Incitement, adaptation, beginning and end fervor, and minor fluctuations all together thus would with the rest give a

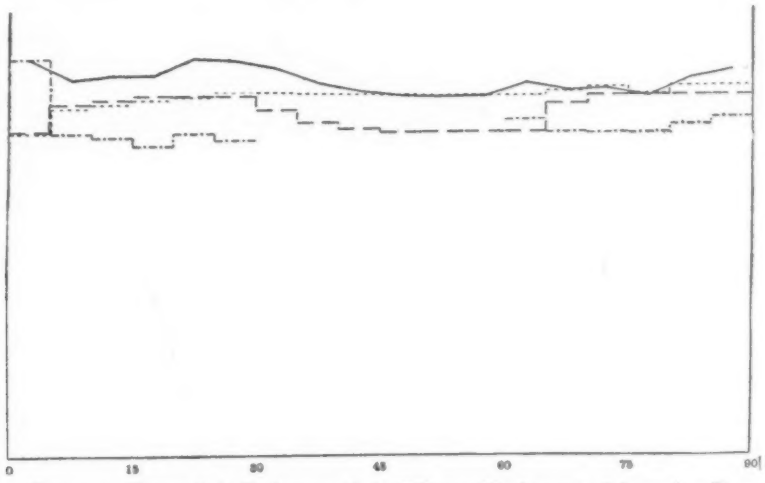


FIG. 13. Kraepelin's Estimates of the Effects of Incitement, Adaptation, Fervor at Starting and Fervor at the Approach of the End during 30 Minutes Continuous Adding, 30 Minutes Rest, and 30 Minutes Continuous Adding. Scaling as in Fig. 12. The Long Dash Line is for Incitement; the Short Dash Line is for Adaptation; the Dash and Dot Line is for Fervor at Starting, Ending, Disturbance, etc.

curve of 483, 448, 464, 463, 483, 480, (rest here) 454, 478, 491, 482, 503, 513, as shown by the continuous line of Fig. 13.

This analysis by Kraepelin is an instructive illustration of the danger of speculative *ex post facto* explanation, for it is certainly highly improbable in almost every one of its main features. If a man of Kraepelin's gifts and training could so

err, others surely can be rarely trusted to imagine the causes of a given curve of work.

The first error in the analysis is the assumption that 30 minute's continuous exercise will add 50 per cent. to efficiency in addition in an educated adult who already adds any one-place to any two-place number in three quarters of a second. No expert in the psychology of practice and of reaction-time would dare to assume so great a gain in so short practice in a function already so far improved.

The second error is the assumption that, in an interval of 30 minutes rest, three fourths of the practice gain is lost. All studies of practice in the case of functions like addition show great permanence of effect after many hours and even days.

The only explanation of their results that would be consistent with any such loss within 30 minutes as Kraepelin here assumes, would be that, even within five minutes or less, there is an enormous decrease in efficiency due to fatigue, so that the net apparent gain from day to day was only a small fraction of what is gained in one day, but lost by fatigue—this in experiments in which the best efforts of the investigator were directed toward avoiding the possibility of any fatigue at all!

A third error is in the assumption that the total practice effect of an hour's addition broken by a 30-minute rest is inferior to that of half an hour's addition alone (plus 188 and plus 199). It might be that in a few individuals no practice effect would come from a second half hour of addition after such rest, but it is enormously unlikely.

A fourth error is the assumption that the fervor of beginning has five times[1] the potency of the fervor from the approaching end of work. There is, on the contrary, a preponderance of evidence to support the belief that, of the two, finishing spurt is more influential than initial spurt in mental work generally and in addition in particular.

A fifth error is the assumption that the fatigue from 30 minutes work will, apart from practice, incitement and adaptation, reduce efficiency by over half. We can reduce the influence of practice, incitement and adaptation by testing indi-

[1] Three times from one point of view.

viduals at or near the limit of practice, and in the second half of a 90-minute period. We do not then find a reduction at all comparable to the assumed 52 or 53 per cent.

A sixth error is the assumption that a half hour's rest will cause seven and a half times as great a percentile loss of the effect of practice as it does of the effect of adaptation! Adaptation, to mean anything distinguishable from practice, should mean an improvement that is *shorter*-lived, that can occur again and again day after day as practice cannot, because it dies out during lack of exercise of the function as practice does not.

The simple fact is that the supposed fatigue of fifty per cent. in thirty minutes and the supposed initial spurt of twenty-five per cent. in two or three minutes are both myths. The enormous practice by a half hour's adding, and the enormous loss of its effects in a half hour's rest, are myths unconsciously invented to support the first myth; the warming-up and the adaptation are modelled into the mythical form necessary to support the second myth.

With increased knowledge and greater watchfulness against inconsistent and improbable suppositions, a psychologist today could improve upon Kraepelin's analysis, but any and all such explanations after the fact are exceedingly risky. In the present state of knowledge it is far better to analyze a work curve by experiment than by deduction. If one thinks that a knowledge of the approaching end produces five per cent. gain in the last ten minutes of an hour, he should have the subject work at the task both with and without knowledge of the approaching end and with all other conditions identical. The difference then found is the effect of knowledge of the approaching end. If certain drops in the curve are thought to be due to disturbances, the disturbances in question should be withheld and applied or increased and decreased experimentally until what difference they actually do make has been measured. The influence of initial spurt, warming up, adaptation, and fatigue upon the early course of the work-curve should be made the subject of speculation only after each of these mythological entities has been defined as some state of affairs in or

out of the organism that can be observed to happen or not to happen, or to happen in varying measurable amounts, and after records have been made of the subject's achievement as related to different amounts and combinations of these states of affairs. At present it is purely fantastic to replace the given fact that in the first twenty minutes such and such a product is achieved, by (*a*) estimates of what that product would have been apart from initial spurt, that is, what work would have been done in the beginning if there had been no beginning, (*b*) estimates of what that product would have been apart from 'warming-up' (that is of what work would have been done at a new task if the task had not been new), and (*c*) estimates of what that product would have been apart from fatigue (that is of what work would have been done if no work had been done)!

COLORED AFTER-IMAGE AND CONTRAST SENSATIONS FROM STIMULI IN WHICH NO COLOR IS SENSED

BY C. E. FERREE AND GERTRUDE RAND

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I. Introduction

In the March number of the Psychological Review, 1907, Thompson and Gordon[1] describe a series of experiments in which colored after-images are obtained in the peripheral retina from stimuli in which no color was sensed. In the November number, 1905, and in the January number, 1908, of the same journal, Fernald[2] working under approximately the same condition reports similar results. In the *Proceedings of the American Philosophical Society*, 1908, however, Titchener and Pyle[3] deny the phenomenon, affirming their complete inability to get colored after-images when no color is sensed in the stimulus.

[1] Thompson, H. B., and Gordon, K., 'A Study of After-images on the Peripheral Retina,' Psychol. Rev., 1907, XIV., pp. 122–167.

[2] Fernald, G. M., 'The Effect of the Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Vision,' Psychol. Rev., 1905, XII., p. 405; 'The Effect of the Brightness of Background on the Appearance of Color Stimuli in Peripheral Vision,' Psychol. Rev., 1908, XV., pp. 33–35.

[3] Titchener, E. B., and Pyle, W. H., 'On the After-images of Subliminally Colored Stimuli,' *Proc. of the Amer. Philos. Soc.*, 1908, XLVII., No. 189, pp. 366–384.

With regard to these discrepant reports, nothing more will be attempted at this place than to point out clearly the position held by each investigator. Titchener and Pyle contend that under no condition known to them can colored after-images be obtained from stimuli in which no color is sensed. Thompson and Gordon, and Fernald claim that under the conditions described by them, after-images may be obtained in which the color is clearly distinguished. The point at issue between them, then, is not whether the phenomenon can be gotten under this or that condition usually obtaining in the work on after-images, or under any given percentage of possible conditions, but whether it may be gotten by any experimental device whatsoever. Nor is it meant to extend the phenomenon to brightness sensation.[1] So far as the writers know, no one has ever claimed to be able to get an after-image from a brightness stimulus too weak to arouse sensation. The question, therefore, whether a subliminal sensory excitation can produce a supraliminal after-effect, considered in a general sense, is in no fashion under discussion. With the issue thus stated, the writers are impelled to take the affirmative side of the question by the experimental results they have obtained, and scarcely less strongly by theoretical considerations of the difference in the effect of different members of the brightness series upon all the colors when fused with them, and in the effect of a given brightness upon different colors. Working under the right conditions, the phenomenon is easily obtained. That its explanation is not essentially difficult will be shown in a later section of this paper.

The work on the peripheral retina has been repeated by the writers and the investigation extended to include other cases in which colored after-image and contrast sensations may be aroused by stimuli in which no color is sensed. It is the purpose of this report to describe these experiments and to explain the results obtained in terms of visual phenomena already known.

Before passing to a description of our own experiments it may be helpful to examine the work of previous investigators in

[1] For the sake of brevity, brightness is used here as a general term for the colorless sensation series, white, black and the grays.

order to determine from neutral ground if possible the cause of the disagreement in the results they have obtained.[1] In general two types of method have been used in arousing the color excitation. In one the color was kept below the limen of sensation by adaptation; in the other it was obscured by the action of an unfavorable brightness excitation. The former method was first used by Tschermak.[2] Tschermak's article is not a description of experimental work. It is an essay in which the author in part seeks to trace the analogies to visual adaptation found in the reactions of other tissue: muscle, nerve, secretory, etc., to external stimuli. The reference to the phenomenon we are discussing is very brief in this article and the description of the conditions under which it was obtained is quite inadequate to serve as a guide for future work. He says: "Haben wir doch gerade in der Anwendung des constanten Stromes auf Nerv und Muskel ein vorzügliches didaktisches Mittel, um die Grundbegriffe der allgemeinen Reiz- und Adaptationslehre zu veranschaulichen und einzuprägen. Am besten demonstrieren wir als Gegenstück zugleich die Wirkung eines mässig satten Farbglases auf das Auge: die Phase der Reizwirkung, individuell verschieden lang, und dadurch erinnernd an die verschiedenrasche Adaptation des Präparates vom Warmfrosch und Kaltfrosch an den constanten Strom—weiterhin das Stadium der vollendeten Adaptation, endlich den gegensinnigen Oeffnungseffect. Nicht minder lehrreich ist die Parallele des subjectiven und des objectiven Erscheinungsgebietes für das Phänomen des Einschleichens d. h. des Ausbleibens einer sinnfälligen Reizwirkung, wenn der Reiz so langsam anwächst, dass das Adaptationsvermögen folgen kann—gleichwohl hat auch nunmehr Wegfall des 'Reizes' eine gegensinnige Oeffnungswirkung. Analoges gilt vom Ausschleichen, also vom Ausbleiben eines sinnfälligen Oeffnungseffectes. Zum optischen Versuche schiebt man zweckmässig successive eine schwach tingierte Glasplatte vor die andere oder benützt einen Keil farbigen Glases."

[1] Because of the inadequacy of Tschermak's description of his method of working this can not be attempted for the work in the central retina.

[2] Tschermak, A., 'Das Anspannungsproblem in der Physiologie der Gegenwart,' *Archives des Sciences biologiques*, Sup. Band, 1904, XI., pp. 79–97.

In 1901 Titchener and Pyle carefully repeated and elaborated upon Tschermak's experiment on the central retina with entirely negative results. The writers can only commend the thoroughness with which they seem to have done this work. Their work on the peripheral retina is introduced with the following words. "We have already mentioned the experiments made by Titchener in 1906 with the view of testing the conclusions of Miss Fernald's first paper. The observations were rigorously confined to the black-white zone, and the outcome was definitely negative. In the meantime, however, the arousal of a colored after-image by a subliminally colored stimulus had been maintained for both the blue-yellow and the red-green zones. Unsystematic observations made in the Cornell Laboratory failed to confirm this result. It seemed worth while, however, to obtain further testimony; and Professor Baird, of the University of Illinois, very kindly consented to investigate the subject. The experiments were carried out by means of a simplified form of the Zimmermann perimeter, which permitted an accurate record of the degree of eccentricity at which the stimulus was exposed. Exploration was confined to the horizontal nasal meridian of each eye. The stimulus was a beam of light from an electric (16 c.p.) lamp, transmitted through appropriate combinations of gelatines and colored glasses; the colors employed were (non-equated) blue and yellow, red and green. Six of the most reliable laboratory students acted as observers, and Professor Baird had personal charge of the entire work. The after-images were projected upon white, gray and black grounds. The experiments proper were preceded by a careful determination of the outermost limits of color vision for the stimuli used, and all pains were taken to avoid chromatic adaptation" (pp. 376–377).

Professor Baird reports negative results in every instance. With regard to this work the writers can not help but observe that Baird has failed to conform to the conditions which Fernald had said are essential for getting the phenomenon. Without drawing upon their own experiments for a knowledge of essential conditions, they will point out three conditions which Baird has apparently failed to fulfill, the neglect of any

one of which is amply sufficient to account for his results being negative. (1) Fernald lays great stress upon the use of a campimeter screen by means of the induction from which the brightness conditions were obtained which obscured the color in her stimulus.[1] Baird used a simplified form of perimeter, how simplified Titchener and Pyle do not state. (2) Baird uses for the duration of the stimulation intervals of 30 to 40 seconds. Fernald is careful to state that the interval of stimulation should not exceed three seconds.[2] (3) In her description of conditions Fernald states that the color should be exposed behind the opening in a campimeter screen, and the card upon which the after-image is projected should be slipped in between the colored surface and the stimulus opening. Thereby the campimeter screen and thus the larger part of the field of vision remains unmoved and the least possible incentive is given for involuntary eye-movement. With Baird's apparatus, however, we would judge that the ground upon which the after-image was projected must have been moved in between the stimulus and the observer's eye, thus exerting a strong incentive to drag the fixation with it. A very slight eye-movement indeed is amply sufficient to blot out or to prevent from developing the instable peripheral after-image.

[1] While, as will be shown later in the article, the writers do not hold as Miss Fernald does that the inductive action of the campimeter screen is an essential or even a favorable condition, still they do insist that a campimeter screen or its equivalent is necessary in order to be able to give a projection field for the after-image without causing an amount of involuntary eye-movement that would prevent the momentary and instable peripheral after-image from developing.

[2] With regard to the importance of this point the writers are in entire agreement with Fernald. In fact one does not need to work long with peripheral after-images to be convinced that so long an interval as Baird used is absolutely prohibitive of colored after-images even when a less excentric portion of the retina is investigated than was the case here. A short interval of stimulation is necessary because of the rapid adaptation of the peripheral retina to color. It is well known that adaptation to color in any part of the retina takes place rapidly at first and then progressively more slowly until a stationary point is reached. Working in the central retina the writers have found a similar curve of effectiveness for after-images. After-images seem to occur most intensively when the stimulus is removed while adaptation is still going on. If one carries the stimulus beyond a stationary point in adaptation, the after-image will weaken roughly in proportion to the length of time during which the stimulus is regarded after the stationary point has been reached. This is true with both intensive and slightly supraliminal stimuli.

Failing to obtain the after-image in the central retina and to get confirmation by Baird that it may be obtained in the peripheral retina, Titchener and Pyle suggest explanations for the positive results gotten by Tschermak and Fernald. "The outcome of Tschermak's observations with the glass wedge must then in our opinion be explained by the prepossession of the observer and the roughness of the method employed. . . . It is less easy to account for the peripheral results." For a full statement of their explanation of the peripheral results the reader is referred to their article, pp. 378 ff. Brief mention only can be made of it here. We wish to comment on three points. (1) Their conception of the problem of getting color in the after-image when none is sensed in the stimulus is, we believe, different from that held and stated by Fernald. "The *experimentum crucis*," they say, "would be the production of a colored after-image in the achromatically adapted eye at a point lying well beyond the limits of blue-yellow vision." It is conceivable that two interpretations might be given to "well beyond the limits of blue-yellow vision." (*a*) The meaning might be well beyond the limits of blue-yellow vision whatever area and intensity of stimulus be used. And (*b*) it might be well beyond the limits at which these colors are seen when the stimuli used in the after-image experiments are employed. Wishing in every case to favor the point criticised, we choose the second interpretation. Thus to obtain a colored after-image well beyond the limits of blue-yellow vision would imply that a negative color excitation sufficiently strong to arouse sensation could occur in response to a stimulus which can under no condition arouse a positive color sensation. This is not at all the claim of Fernald nor of Thompson and Gordon as we understand their claim. They believe, in the cases cited by them, that the stimulus was prevented from arousing positive sensation by unfavorable brightness conditions. While it may not be clear from their work how these unfavorable brightness conditions prevent the positive sensation and permit the negative, there can hardly be any doubt that they would not claim that colored after-images can be aroused in the retina at a point "well beyond the limits of color vision." In fact in her

paper of 1905 Fernald specifically states that one should work just within the limits of color vision.[1]

(2) It is pointed out by Titchener and Pyle that the limits obtained by Miss Fernald show a considerable range of variability. The "*experimentum crucis*" thus has been inadequately performed. She has in reality obtained the after-image within the limits of blue-yellow vision. As pointed out in (1), this, we believe, is not the point at issue. The question is not whether the after-image can be obtained beyond the limits of color vision, but whether it can be obtained anywhere in the retina when color is not sensed in the stimulus in a percentage of cases large enough to preclude the possibility of its being due to chance happening, to error of observation, or what not. Apparently neither Fernald nor Thompson and Gordon, and certainly not Tschermak, have entertained the idea that the colored after-image can be obtained at any point on the retina where experiment shows that color can not be obtained in the positive sensation for the given stimulus under any conditions whatever.

(3) Titchener and Pyle quote the following observations furnished by Miss Fernald by private correspondence.[2]

[1] In her paper of 1908 Fernald states that the after-images are perceived most frequently just inside or just beyond the regular limits for the color. As compared with her first statement, this may seen somewhat loose and might lead to misunderstanding. There can be little doubt, however, that beyond the regular limits means for her beyond the limits obtaining for some given set of brightness conditions taken as standard—not beyond the limits for all brightness conditions. Fernald, it will be remembered, worked with very little control of the factors extraneous to the source of light that influence the sensitivity of the retina to color. The boundaries of her color zones thus varied within quite wide limits. In this region through which the boundaries of the zones varied, she was able to obtain color in the after-image when none was sensed in the stimulus. Her 'regular' limits fell somewhere within this region. She was thus able to obtain the after-image sometimes just within, sometimes just beyond the regular limits. She apparently never obtained the after-image at a point beyond her widest limits of sensitivity to a given color. The present writers, working with a control of brightness conditions that enabled them to duplicate their limits from sitting to sitting within a degree of variation, never obtained the after-image beyond the limits determined under their most favorable brightness conditions.

[2] A request was sent to one of the present writers (Ferree) to determine whether these after-images could be gotten, at the same time the similar request was sent to Dr. Baird. The results quoted are samples of the observations made at that time.

Observer: C. E. Ferree. Full illumination on bright day (May 17, 1908). Nasal meridian, right. White ground. Projection field white, except in obs. 14–17, when it was black. Stimulus, 13 sq. mm. Distance from eye to stimulus, 25 cm.

Fixation Point	Stimulus	Color Seen	After-image
80°	*O*	Dark gray	Unsaturated light blue
85°	*B*	Just dark	Wash of unsaturated yellow
85°	*Y*	Nothing	Nothing
80°	*Y*	Tinge of dirty yellow	Very pale blue
80°	Medium gray	Dark	White
80°	*O*	Indefinite gray	Nothing
80°	Light gray	Dark	White
75°	*Y*	Reddish yellow	Good blue
75°	*B*	Good blue	Good yellow
75°	*B*	Good blue	Good yellow
65°	*O*	Yellowish red	Unsaturated blue
65°	*Y*	Reddish yellow	Blue
60°	*G*	Indefinite greenish gray	Uncertain
65°	*G*	Greenish yellow	Dark red, more saturated than stimulus
80°	Medium gray	Dark	Nothing
80°	Medium gray	Dark	Nothing
65°	*G*	No color	Flash of red
65°	*R*	No color	Blue

Commenting on these observations Titchener and Pyle say: "Positive results occur in the first two and last two observations of the series. The former may be explained in terms of chromatic adaptation. If as the illumination suggests the observer began the work in yellow adaptation, the first blue after-image would naturally follow." If the above explanation were adequate, the blue after-image should have been obtained also when gray was used as stimulus. As a check experiment gray was repeatedly used as stimulus by Fernald, and in no case was the characteristic blue after-image obtained.[1] Furthermore the same yellow light which in terms of the explanation gave the stimulation for the blue after-image fell with undiminished intensity on the projection field, hence there was no shutting off and, since the projection field was white, even no diminution

[1] In a footnote p. 382 we find: "These observations were taken after the limits had been roughly determined in previous experiments. If the determination of limits was made at the same sitting, and if the last test color employed was orange, there would be additional reason for an initial yellow-adaptation." In reply to this we would say that in case of our own experiments orange and yellow were purposely not the last colors used in determining the limits; that had they been, abundance of time was given for the complete recovery of the eye before the after-image experiments were performed; and that if chromatic adaptation had been present, its effect should have shown in the after-image when gray was used as stimulus.

in the amount of the yellow daylight reflected to the eye for the period during which the after-image was observed. Thus no chance was given for a blue after-image to develop as the result of stimulation by the yellow in the daylight. Continuing Titchener and Pyle say: "If the second observation was taken at too short an interval of time the resulting blue-adaptation should show itself as a yellow after-image." We interpret this statement with some hesitation. It seems to mean that the first observation gave a blue after-image, and that if the second observation followed too closely upon the first, this after-image excitation in turn aroused a negative excitation which formed the physiological basis of the yellow after-image observed. If this interpretation is correct two conclusions would logically follow. (1) A negative after-image can itself give a negative after-image,—a phenomenon which, so far as the writers know, has never yet been observed. And (2) although a colored stimulus too weak to arouse positive sensation can not arouse a colored after-image, still the excess of yellow in clear daylight, which, reflected from the yellow stimulus, is too weak to be sensed as color, can arouse an after-image which not only can be sensed as color but which in turn can arouse a second after-image in which color can be sensed. "The two final observations suggest a shift of conditions. Green is seen at 65° as greenish yellow and at 60° as indefinite greenish gray. It is possible that in the case in which 'no color' is reported the green simply escaped notice; peripheral colors at the limit of vision often appear as momentary flashes. Again red is reported at 65° as 'no color' although reddish yellow had been seen as far out as 75°. It is possible that the flash of red escaped notice; it is also possible that red-adaptation from the preceding after-image brought out the blue." With regard to the possibility of the red and green escaping notice, the following points may be noted. (1) The stimulus color in this region of the retina has not so much a momentary character as the after-image color and would, therefore, not be so likely to escape notice as the after-image color. (2) The observations quoted above were made by one of the writers (Ferree), who here positively asserts that to the best of his knowledge this

was not the case. (3) In our own experiments to be described later in this article, conditions were obtained under which the after-images of red and green were obtained in practically 100 per cent. of the observations made. It seems scarcely possible that the color should have escaped notice in the stimulus in all of these cases and have been observed in the after-image. In case it be held that "red-adaptation from the preceding after-image brought out the blue," we are again asked to accept the thesis that an after-image excitation may in turn give rise to an after-image excitation strong enough to be sensed as color.

II. Cases in Which After-Images and Contrast Sensations are Aroused by Retinal Excitations Which Do Not Directly Condition Sensation

1. *After-Images*

The problem presented is: Can the color in a stimulus be obscured directly for sensation, and still set up an excitation upon the retina which will give an after-image? Our answer is that it can be done both in the central and the peripheral retina, but possibly more readily in the latter than in the former. To accomplish it, an experimental condition must be devised which will work against the color in the stimulus and relatively favor it in the after-image. A working principle is found in the difference in effect of brightness changes upon the saturation of the colors.

This effect may be expressed as follows. (1) Brightness fused with color inhibits the color sensation. With the exception of the region just within the limit of sensitivity for two colors,[1] the following may be stated roughly as the law of this action for all colors for all parts of the retina. White inhibits

[1] Over a region 3° in width just within the limits of sensitivity as determined with the Hering pigment papers at full illumination, red and yellow have a higher limen in black than in white. In this zone red and yellow darken as compared with their brightness values in the central and paracentral retina. Added to the black of the disc, then, we have the black due to the darkening of the color. Thus the figures which are read from the disc do not express the actual amount of black added to the color. Whether or not we have an exception here to the law of the action of brightness upon color which obtains in the rest of the retina is thus open to question. At least, the exception as expressed by the measurements is exaggerated.

most, the grays in the order from light to dark next, and black the least. (2) A given brightness change does not affect all colors to the same extent. If local and towards either black or white, blue and green lose their saturation completely before red, orange, and yellow; when the change is general and toward black, produced by decrease of illumination, yellow, red, and dark orange are obscured before green and blue. All of these changes were utilized at different times in our experiments both on the central and peripheral retina. It is readily seen that the technique of getting a colored after-image from a stimulus in which no color is sensed becomes merely a matter of fusing the least favorable brightness quality with the stimulus color and the most favorable with the after-image color. When this technique was carried out in its best form, the colored after-image was obtained in practically every case.

i. In Central Vision

Two methods of working were used in these experiments, one in which the brightness control came through changes in the general illumination of the field of vision, and the other in which the changes were local. The former method will be described first.

After-images were obtained of red, yellow, and orange stimuli of the Hering series of colored papers. The work was done in a long narrow dark-room with a small window at one end near the center, darkened by a solid and carefully padded door. By opening and closing this door, the general illumination of the room could be varied at will. The observer was seated in front of the window and to the left, so that the light coming in from above and behind fell free from shadow upon a screen placed in front of him.

The intensity of the stimulus was decreased by decreasing the illumination. Two methods were used in doing this, differing in the amount of dark-adaptation the eye had experienced before the stimulus was given. In the first, a preliminary determination was made of the illumination at which the stimulus was sensed as colorless. The experiment was then conducted at that illumination. The stimulus was exposed

from 5 to 15 seconds, varying with the subject, and the after-image observed. In the second method, the experiment was started at full illumination. The door was opened and the stimulus covered with a card of the same gray as the background. The door was then closed rather quickly until the point was reached at which the color is unperceived, this point having previously been carefully determined. The stimulus was exposed before the eye had become adapted to the decreased illumination, and the after-image was observed. The advantage of the second method is that with it the eye loses its sensitivity to color at a more intensive illumination than when the first method is used. That is, the eye gains in sensitivity to color at any point in a series of decreasing illuminations if it is allowed to adapt to that illumination before the exposure is made. Thus, by taking advantage of the lessened sensitivity of the eye to color with decreased illumination before adaptation sets in, we were able to work at an illumination that gave a stronger objective stimulus, and was more favorable for the development of the after-image. Either method, however, gave us unmistakable color in the after-effect. In order that we might be sure that the observer worked with stimuli in which the color could not be sensed and that color really was distinguished in the after-image, he was in the first place kept in ignorance of the color that was to be used in a given test, and in the second place was given at intervals in the series a gray for stimulus which could not be distinguished from the color at that illumination. Thus having run blank tests, and having used every precaution known to us to eliminate the influence of expectation, etc., we feel a reasonable degree of confidence in the results obtained.

The after-images gotten in this way in central vision are not so saturated as those obtained in peripheral vision, but in proportion to their saturation last much longer than peripheral after-images. The after-image of red shows greatest saturation, dark orange next, and yellow least. As one would expect from its quicker rate of adaptation and from the fact that after-images decrease in intensity after the stationary-point in adaptation is reached, the time of stimulation most favorable for

producing the after-image was shorter for the red stimulus than for the other colors. The time for orange was next shortest, and for yellow the longest.

Local changes in the brightness of color may be produced by objective mixing, by contrast, and by after-image. Of these three methods the addition of brightness as after-image is by far the most successful. By objective mixing the amount of colored light coming to the eye is reduced in proportion to the amount of brightness added. The color thus loses saturation from two causes: the reduction of the amount of colored light coming to the eye, and the inhibitive action of the brightness excitation upon the color excitation. For our purpose, it is of advantage to obtain the loss in saturation without any reduction of the amount of colored light coming to the eye, *i. e.*, by means of the inhibitive action of the brightness excitation alone.[1] This can be done by the addition of the brightness either as contrast or as after-image. Of these two, the after-image method yields the better results, as much more brightness can be added as after-image than can be induced as contrast. This is especially true of the central retina. The amount of inhibitive action that can be obtained by this means may be shown as follows. The limen of Hering red in a gray of its own brightness is for one observer 27°. When the after-image obtained by a stimulation of 10 seconds to black is projected on this color, the limen is raised to 60°. This after-image decreases in intensity very little for the first 8 to 10 seconds, and disappears after from 12 to 15 seconds. 10 to 12 seconds stimulation to color is quite sufficient to arouse an intensive colored after-image. Hence by a preëxposure of 10 seconds to black and by the projection of the after-image upon a colored stimulus, it is possible to keep the color in a fairly intensive colored stimulus below the limen long enough to arouse the excitation necessary to give a colored after-image of considerable intensity.

The method of working is as follows. A small colored disc composed of sectors of the color to be used and a gray of the brightness of this color, is set up on a color-mixer. A second

[1] As will be shown in a later paper, this action inhibits the effect of the color excitation, although it does not reduce its power to arouse after-image and contrast.

disc of black of the same dimensions is set up beside it. As much color is put in the first disc as will be rendered just subliminal by the after-image obtained by a 10 second stimulation to the black disc. The colored after-image is then projected on a black field. The addition of white to the stimulus works against its color, and the black projection field favors the color of the after-image. Proceeding by this method, a colored after-image of good saturation and duration can be obtained in every case. The method can be made still more effective by adding the brightness to the stimulus color by both contrast and after-image. For example, the colored disc may be exposed through an opening in a black screen. The white induced by this screen would then add to the effect of the white after-image and in consequence still more color may be used as stimulus without arousing a color sensation.

ii. In Peripheral Vision

The color in the stimulus may be obscured in three ways. (*a*) It may be carried to the peripheral retina near the limit of sensitivity for the color used and a brightness be induced across it that works against its saturation. (*b*) The unfavorable brightness may be mixed with it as after-image aroused by preëxposure to a brightness stimulus. (*c*) The stimulus may be carried to some angle of indirect vision not too remote from the fovea and the general illumination be decreased until the color is lost. In (*a*) and (*b*) the brightness is mixed with the color by contrast and after-image rather than added to it on the color-wheel or by some other means of objective mixing, because, as stated before, objective mixing decreases the physical intensity of the stimulus. This would decrease the energy of the positive color excitation, which would in turn decrease the energy of the negative excitation, and thus defeat the purpose of the experiment. But the addition of the brightness as contrast or after-image does not affect the energy of the stimulus; and while it reduces the effect of the positive excitation upon sensation, it apparently does not decrease its energy as retinal excitation, for it does not lessen its power to arouse after-images. This strongly indicates that the action of bright-

ness upon color takes place at some physiological level posterior to the seat of the positive and negative color processes, as will be shown in the next paper of this series.[1] Consider method (*b*) for example. Here the brightness is added as after-image. Its effect is to blot out the weakly supraliminal color in the stimulus, but it does not prevent the complementary color from appearing in the after-image. This is readily explained in terms of the above hypothesis as to the level at which this action takes place. If, as we suppose, the inhibition takes place posterior to the level of the positive excitation, the negative excitation is not weakened thereby. And since the brightness after-image which was added cannot itself leave behind a brightness after-excitation, nothing is carried over to the negative color excitation to weaken its effect upon sensation. Hence by this method of working we should get as much color in the after-image as if no brightness quality had been added to the colored stimulus.

While method (*a*) can also be used to obscure the stimulus color, still it is not so effective as (*b*) for obtaining the colored after-image; because (1) by means of it the zone of sensitivity cannot be narrowed nearly so much as by (*b*), and (2) the inducing field throws by contrast the same brightness quality across the after-image as it does across the stimulus, and so, roughly speaking, it inhibits the after-image as much as it inhibits the stimulus.[2] In fact, in the way it has to be used in the peripheral retina method, (*a*) alone is of little use in getting our phenomenon. To make this method work successfully, some means would have to be devised for changing the quality of the inducing surface in the interval between the end of the stimulation and the beginning of the after-image. That is, the stimulation would have to be given on a field which induced a brightness quality unfavorable to the saturation of its color,

[1] See 'An Experimental Study of the Fusion of Colored and Colorless Light Sensations: The Physiological Level at which this Action Takes Place.' (In press.) See also abstract in *Journ. of Phil., Psych., and Scientific Methods*, 1911, VIII., p. 294.

[2] The second objection to this method does not apply to the use of the contrast method in the central retina (see p. 207), for in the central retina the colored after-image is sufficiently stable and durable so that an entirely new projection can be substituted for the screen which induces the contrast. In this case the effect of the induction of this screen can be eliminated from the after-image.

and the after-image observed on a field whose inductive action was favorable to the saturation of its color. This change of the inducing field, however, can not be made for two reasons. (1) The duration of the peripheral after-image is very short. It comes as a momentary flash of color immediately after the stimulus light is shut off, and disappears before a change in the inducing field can be made. And (2) the after-image would be completely extinguished by the eye movement set up by shifting so large a part of the field of vision.[1]

The investigation of favorable conditions was conducted in part by means of the vertical campimeter and in part by means of the rotary campimeter.[2] The campimeter provides three possibilities for the variation of brightness conditions which was needed in our problem. By means of it, the brightness of the local preëxposure, the brightness of the inducing field, and the brightness of the field on which the after-image is projected may be changed at will. The brightness of the local preëxposure and of the field on which the after-image is projected may be varied by changing the cards held in the frame behind the stimulus opening in the campimeter screen. The brightness of the inducing field may be varied by changing the paper covering the campimeter screen. All of these variations were used in our investigation. Fernald recognized the influence of only two of these variations: the campimeter screen and the projection field.[3] And of the two the importance of campimeter screen was very much overestimated, while the impor-

[1] See Ferree, C. E., 'The Fluctuation and Duration of the Negative After-image,' *Amer. Jour. of Psychol.*, 1908, XIX., pp. 68, 87–97.

[2] For a description of the vertical campimeter see Fernald, G. M., 'The Effect of the Brightness of Background on the Appearance of Color Stimuli in Peripheral Vision,' PSYCHOL. REV., 1908, XV., pp. 27–29. For a description of the rotary campimeter see Ferree, C. E., 'A Description of a Rotary Campimeter,' *Amer. Jour. of Psychol.*, 1912, XXIII. (In press.)

[3] The influence of preëxposure under the ordinary conditions of working was apparently not at all realized by her. Her preëxposure and projection field were in most cases made of the same brightness as the campimeter screen. No reason is assigned for doing this. Some indication of her reason may perhaps be had from Thompson and Gordon who also use a preëxposure and projection field of the same brightness as the campimeter screen. They say: "When the stimulation had lasted the desired time, the screen was again put over the color, thus making with the campimeter a uniform gray surface upon which the after-image could be observed" (PSYCHOL. REV., 1907, XIV., p. 124).

tance of projection field was relatively underestimated. Moreover she seemed to entertain no clear notion of just how these factors produce their results. For example, the brightness induced by the campimeter screen was supposed to contribute in some fashion to the production of her phenomenon by working against the color in the stimulus and favoring it in the after-image; but just how it operates to do this was left in question. Apparently she had made no quantitative study of the fusion of brightness with color and of the relative influence of the different brightness qualities upon the saturation of color. From the side of explanation, then, she can scarcely be regarded as having a definite point from which to start.

Since the writers report the after-image in so much greater percentage of cases than Miss Fernald, and differ from her so much in their explanations of results, it may be well to discuss her work at this point in greater detail.[1] They have the following comments to make on her general technique and her explanation of how it produced the results she obtained.

1. No systematic attempt was made to determine the factors influencing her phenomenon. Of the full list of achromatic factors, for example: brightness of the surrounding field, brightness of the preëxposure,[2] brightness of the projection field, and the degree of general illumination, she recognizes only brightness of the surrounding field, brightness of the projection field, and the degree of general illumination. Especial stress is laid on the brightness of the surrounding field. As we have already shown, the influence of brightness of surrounding field is comparatively insignificant because it is exerted both upon the color of the stimulus and the color of the after-image. Thus if it is unfavorable to the one it will also be unfavorable to the other. It has a margin of influence, however, due to the fact that it does not act with equal strength upon the stimulus and the after-image. Its action is unequal because stimulus and after-image differ both in color and brightness. There would be on this account a small difference in the amount of induction by the screen in the two cases, due to the difference between the brightness relation of stimulus to background and that of after-image to background; and further a probable difference in the

[1] Fernald's work rather than Thompson and Gordon's is singled out at this point because Fernald has made a more extensive investigation of the brightness factors and their relation to the phenomenon in question than have Thompson and Gordon.

[2] Miss Fernald always used some kind of colorless preëxposure, but apparently she did not recognize its importance as a brightness factor. For her the preëxposure card served in the main merely as a neutral covering for the colored stimulus until a steady fixation could be obtained and the observer be otherwise made ready for the exposure to color. Towards the end of the work reported in 1909, however (p. 29), she apparently began to realize that if the fixation were held for a period varying from three to ten seconds, the preëxposure card would give a brightness after-image which would mix with the stimulus color and lighten or darken it, as the case might be. But under the ordinary conditions of working, she took no account of it as a brightness factor.

inhibitive action of this induced brightness upon the color, due to the fact that the stimulus and after-image are different colors. The factors of predominant importance are the brightness of the preëxposure and of the projection field. These are of greater importance (*a*) because more brightness influence can be exerted by means of them; and (*b*) because their action is more differential, *i. e.*, the brightness of the preëxposure acts upon the stimulus alone, and the brightness of the projection field acts upon the after-image alone. The brightness of the preëxposure can thus be made to work against the color in the stimulus and to have no effect upon the color in the after-image, and the brightness of the projection field can be made to favor the color in the after-image and to have no effect on the color of the stimulus.

2. No attempt was made to separate the achromatic factors and to study directly the relative importance of their effect upon the frequency with which the after-image may be obtained. However, in her study of the effect of achromatic conditions upon the limits of color sensitivity and upon color quality in stimulus and after-image, some attempt at separation was made, and in these experiments the observer was required to report cases in which color was sensed in the after-image when none was sensed in the stimulus. In order to study the effect of a given factor, the influence of all the factors but the one to be studied should be eliminated from the conditions of the experiment. If, for example, it were wanted to study the effect of the projection field, a gray of the brightness of the colored stimulus should have been chosen for the campimeter screen and preëxposure. In no case was this method of working employed. In her experiments to determine the effect of projection-field[1] the following conditions were used: The campimeter screens were platinum white, and black; the projection fields for each screen were in turn of black, white, and medium gray; and the preexposure was in each case of the same brightness as the projection field. Since neither the preëxposure nor the screen was chosen of the brightness of the colored stimulus, all the factors were present in each case, instead of one. And in her experiments to determine the effect of surrounding field, screens of white, black, and gray of the brightness of the color were used with preëxposures and projection fields to match the screen in each case. In this case also it will be observed that when the brightness of the screen was varied the influence of neither of the other two factors was ruled out.

3. Her statement of the most favorable brightness conditions for getting the after-image is in complete contradiction to all we have been able to find out directly concerning the phenomenon, or to infer from our investigations of the action of brightness upon color either in the positive sensation or in the after-image. For example, she states that white campimeter screen and a white projection field give the most favorable conditions for obtaining after-images of blue and yellow when no color was sensed in the stimulus. Stating her most favorable conditions for the after-images of red and green, she says: "In several instances in our later work a red after-image has followed an unperceived green when the stimulus was given on a white background and the dark screen [projection field] pushed over the color, and a green after-image was obtained for red and orange when the projection-screen was middle gray or black."[2] The writers were able to obtain after-images of the above colors under the conditions cited by Miss Fernald as most favorable, but instead of finding them to be the most favorable they found them to be almost the most unfavorable. As stated above, their most favorable conditions were black preëxposure, black campimeter screen, black projection field; and their most unfavorable conditions were white preëxposure, white

[1] Fernald, G. M., PSYCHOL. REV. MONOG., 1909, X., pp. 37–45.

[2] *Op. cit.*, p. 82.

campimeter screen, and white projection field. The variation of the factors between these extremes gave the phenomenon with a frequency ranging between maximal and minimal. Working with the conditions she found to be most favorable, Miss Fernald was able to obtain the after-image in only approximately 31 per cent. of the total number of cases.[1] With the conditions we have found to be most favorable the after-image may be obtained in practically every case.

4. Miss Fernald's explanation of her results is also in contradiction to the facts as we find them. (*a*) She says that the most favorable conditions are gotten when the "projection-screens were determined so as most to emphasize the after-image color and the background least to favor the stimulus color."[2] The campimeter screen, then, that narrows the zone of sensitivity for a given color furnishes the most favorable conditions to be obtained for that color. The white screen is most favorable because "in so far as our results justify any conclusions concerning the color limit they seem to show that all the colors except the reds are perceived at a greater degree of eccentricity with the dark than with the light backgrounds. Red is seen as red to about the *same* degree of eccentricity with the dark and light backgrounds, but it is seen as yellow or orange with the dark background at the same points at which it is seen as colorless with the light background."[3] Her exception in case of red seems due to the fact that the limit for red is for her where first a trace of yellow comes in. If the limit for red had been taken at the point where the last trace of red is seen, as is usually done, red would have proved no exception to the other colors, and we could derive from her results the law that all the colors have a narrower limit with the white screen than with the dark or black screens. We do not find this to be true. For us blue and green have a wider limit with the black screen than with the white, and red and yellow a narrower limit. These results were obtained with the effect of preëxposure eliminated and with the illumination of the optics room carefully standardized by a method to be described in a later paper. Neither of these precautions was observed in Fernald's work. Their importance may be shown by the following results. As compared with the gray of the brightness of the color, a preëxposure of three seconds to black was found by one of the writers (Rand) to narrow the limits of sensitivity for red, green, and yellow 6°, and for blue 11°; a preëxposure to white to narrow the limits for red 5°, for green 2°, for yellow 4°, for blue 7°. At an eccentricity of 35° on the temporal meridian, a preëxposure of three seconds to black raised the limen for red 58°, for green 80°, for yellow 32°, and for blue 13°; a preëxposure to white raised the limen for red 45°, for green 55°, for yellow 12°, and for blue 7°. When the white campimeter screen was used, the changes in illumination from 11 A.M. until 4 P.M. on a bright day in September were found to vary the limits for the different colors from 4° to 6°. The change in the illumination of our optics room, lighted by skylight and provided with diffusion sashes to lessen the effect of external changes, from a bright morning to a cloudy afternoon was sufficient to vary the limits with a white screen from 6° to 14° for the different colors; and with a black screen from 2° to 7°. The greater variability is found for the white screen because the amount of contrast it induces is found to vary more with the change of illumination. (*b*) Miss Fernald seems to believe that the brightness process may have either a stimulating or an inhibiting effect on the color process. She says: "There seem to be two possible ways of explaining the action of brightness: Either the brightness of the stimulus has a direct inhibitory or stimulating

[1] Fernald, G., PSYCHOL. REV., 1908, XV., p. 33.

[2] Fernald, G., PSYCHOL. REV. MONOG. SUP., 1909, X., p. 82.

[3] *Op. cit.*, p. 23.

effect on the color processes, or the brightness primarily affects the brightness substance, and the activity in brightness substance has some differential effect on the color activity. . . . The fact that our most striking effects were obtained when the brightness is superimposed on the color, *i. e.*, when the brightness is largely determined by contrast with a brightness background, or when the after-image is projected on a light or dark ground, seems at least to justify the statement that the superimposed brightness acts in such a way as to inhibit, increase, or modify the color activity."[1] Her claim that all the colors have their widest limits with the black screen and their narrowest with the white seems to indicate that she considers that white increases the color activity and that black decreases it. She says: "This brightness factor is effective to a very limited extent in central vision, to a much greater extent in peripheral vision. As we go out into the peripheral retina, the action of the white process is needed more and more, *i. e.*, the colors must be made lighter to be most strongly sensed."[2] Our results show that the brightness process, white, black, or gray, must be considered as always having an inhibitive action on the color processes. When the brightness process is added to the color process either as after-image or contrast, the amount of colored light coming to the eye remains unchanged and yet the saturation of the color is considerably decreased. Instead of being increased by the action of white and decreased by the action of black the saturation is decreased by the action of both, but much the most by the action of white for all colors for all parts of the retina with the exception of for red and yellow within a very narrow zone just within the limits of sensitivity. In this zone red and yellow have a higher limen in black than in white. It is not certain, however, that even in this zone black exerts the greater inhibitive action, for it is difficult to add the same amounts of black and white to red and yellow because these colors darken in the peripheral retina and it is hard to get a measure of how much black is added by this process alone. The relative amounts of inhibitive action by white, black, and gray of the brightness of the color can also be shown by the method of objective mixing. When equal amounts of white, black, and the gray are added in turn to a disc of color on the color mixer, the colored light coming to the eye is reduced an equal amount in each case, yet the apparent saturation of the color is very different on the three discs. It is much the greatest on the disc to which black has been added, next greatest on the disc to which gray has been added, and the least on the disc to which white has been added. Or to get a more exact measurement of the actions in each case, the color limen may be determined. An amount of color which is just noticeable when added to the black disc will give no color at all when added to the white or gray disc. A considerably larger amount must be added to the gray disc to give color, and a still larger amount must be added to the white disc than was added to the gray disc.

Such a confusion as Miss Fernald and many others before her have fallen into with regard to the relation of brightness to intensity or strength of color is apt to come from the failure to separate the action of the brightness or achromatic excitation from the action of physical intensity or energy of the light waves coming to the eye. When a colored light of low energy strikes the eye it is both unsaturated as to color and for the most of the colors of low brightness. As its energy is increased it becomes more strongly sensed as color and in most cases lighter. That the two changes go hand in hand when the physical energy of the colored light is changed, however, does not justify the inference that they will go hand in hand when there is no change in the energy

[1] *Op. cit.*, p. 79.
[2] *Op. cit.*, p. 73.

of the colored light—in other words does not justify the inference that an increase in the white excitation will heighten the color excitation. The central principle of Miss Fernald's method of working is to change the achromatic excitation without affecting the amount of colored light coming to the eye. In all such cases, with the possible exception of red and yellow for the small zone mentioned above, the saturation of the color decreases rapidly as the quality of the achromatic component is made lighter. In addition it is scarcely necessary to point out that just because a color reaches its maximal saturation at a given brightness as the energy of the colored light is increased, it does not follow that the achromatic excitation corresponding to this brightness is the most favorable in its action upon the color process. When the amount of colored light is rendered constant and the achromatic factor is varied, it is found that black is the most favorable quality of the achromatic series and the specific grays are favorable in proportion as they are near to black in quality. The gray of the brightness of yellow, for example, kills out the color in yellow when mixed with it almost as rapidly as does white and much more rapidly than does black and the darker grays. Thompson and Gordon, while in general agreeing with Fernald with regard to the effect of brightness relations, in one place seem both in addition and in contradiction to have fallen into this latter error. They say: "The effect of the background then seems to be this; that in a colored after-image, that color element is emphasized which in brightness approaches the brightness of the background [by background is meant here the field on which the after-image is projected], that is, on the lighter grounds the brighter element comes out and on the darker grounds the darker color element."[1]

By way of clearing the ground for explanation, the writers have made a detailed study of the influence of the various brightness qualities upon color in the central retina, and for a large number of meridians in the peripheral retina. Since a statement of the results obtained will be given in full in a later paper, nothing more than a general statement will be attempted here. In the central retina, white reduces the saturation of colors the most, the grays in the order from light to dark next, and black the least. In the peripheral retina, this law holds for blue and green out to the limit of sensitivity for all of the observers worked with; and also for red and yellow for all of the peripheral retina except a very narrow zone just within the limit of sensitivity. In this zone, black apparently inhibits red and yellow most strongly—at least, red and yellow have a higher limen here when mixed with black than when mixed with white or the grays. An ultimate statement of most favorable conditions for all parts of the retina, then, would have to take into consideration all of the facts. It would involve a more detailed consideration of the topography of the retina than can be gone into in this paper. In formulating our

[1] Thompson and Gordon, *op. cit.*, p. 128.

experimental technique, however, we have avoided, we believe, the difficulty raised by the exception to the law found just within the limit of sensitivity by employing inhibitive conditions sufficiently strong to allow us to work nearer the center of the retina, where the exception has never been found. Thus we have been able to use as our working principle the law that white inhibits the most, grays in the order from light to dark next, and black the least. But to determine in accord with the law even thus simply stated, the brightness quality of preëxposure, campimeter screen, and projection field that will give the conditions most unfavorable for the stimulus color and most favorable for the after-image color, is not easy. For example, in order to inhibit the stimulus color most strongly, the preëxposure must be black, so as to give a white after-image to fuse with the color. This effect could be strongly intensified by having the black preëxposure made through an opening in a white campimeter screen, and the color exposure, which comes immediately after and simultaneously with the after-image of the black preëxposure, made through a black screen. This would secure the greatest possible intensification of the white after-image, and, therefore, the greatest possible amount of inhibition of the stimulus color. In order to favor maximally the saturation of the after-image color, the after-image should be projected on a field of very dark gray or black.[1] As to campimeter screens to be used during the projection of the after-image, we have a choice again of brightness qualities ranging from white to black. White and the grays in proportion to their whiteness would intensify by contrast the blackness of the projection field, while black would exert little if any influence. The black screen, then, is probably the safest to use while the after-image is being observed for two reasons. (1) If one is working with a general illumination at all intensive, white and the grays in proportion to their whiteness induce enormously in the peripheral retina. This amount of induction of black carries the brightness quality beyond that

[1] It may be well to state that in our study of the effect of brightness upon color, the black used was the matt black of the Hering papers. When we state that black favors the saturation of colors, this black is referred to.

specified in our law as most favorable, namely, the blackness of the pigment of the Hering paper. (2) There is always danger that we may not be working far enough within the limit of sensitivity to escape the exception to our law of most favorable action. If, then, our formulation of conditions be correct, we should have a white campimeter screen during preëxposure to black, and a black screen during both the exposure of the stimulus color and the projection of the after-image. This would involve two changes of the card behind the stimulus opening in the screen, and one change of the screen. The change of cards causes no disturbance in our phenomenon, but a change of the campimeter screen in the interval between the preëxposure and the exposure to color would be fatal to the success of our experiments. This is because the white after-image would not last through the change, for reasons that have already been discussed; and the effect of the preëxposure on the stimulus color would therefore be lost. We are thus limited to one screen for a single experiment, and our problem becomes to determine which of the brightness qualities acting continuously through all three stages of the experiment will be the most favorable for our phenomenon. After rough preliminary tests, three screens were selected as representative of the action of all, namely, white, black, and a gray of the brightness of the color to be used. Of these three, the black screen was found to be much the most favorable. A consideration of the action of the three brightness qualities upon pre-exposure, stimulus color, and after-image shows sufficient reason for this. The effect of each is as follows: (1) The white screen intensifies the blackness of the preëxposure, darkens the stimulus card, and, in the peripheral retina, especially if the general illumination is intensive, piles up the blackness on the projection field to a degree that is unfavorable to the saturation of the after-image color. (2) The gray screen of the same brightness as the color adds blackness both to the preëxposure and to the projection field, but not so much as is added by the white screen. It has no effect on the stimulus card. (3) The black screen has no effect on the preëxposure; it induces white on the stimulus card and thus adds to the

effect of the preëxposure on the stimulus color; and it exerts little or no effect on the projection field. A comparison of these effects shows that the black screen in all probability inhibits the stimulus color more than any of the others, and is less unfavorable in its action upon the after-image color. For this reason, it gives the most favorable brightness conditions for obtaining our phenomenon. Considering all the factors, then, we find that apparently the most favorable combination that can be made for our purpose is black preëxposure, black campimeter screen, and black projection field. The results of our experiments show this to be true. Under these conditions, color was obtained in the after-image in practically every case. The next most favorable condition was given by the white or gray screen with black preëxposure and black projection field. The poorest results were obtained with a white preëxposure and white projection field. With this combination, color could not be gotten in the after-image with any consistency of result, whatever brightness quality was used in the campimeter screen.

Having thus worked our way through an explanation of our phenomenon and a determination of the conditions under which it can best be obtained, we will devote the remainder of the report of the work done by methods (*a*) and (*b*) to a brief review of the results obtained in support of the various points that have been made. Our general thesis was that the phenomenon under consideration is but a special case of the difference in the inhibitive action exerted upon color by the different brightness qualities. Color may be obtained in the after-image when none is sensed in the stimulus, if an unfavorable brightness quality is fused with the stimulus color and a favorable one with the after-image color. In detail, our first point was that for all parts of the retina and for all colors, with the exception of two over a narrow zone just within the limits of sensitivity, white reduces the saturation of color the most, the grays in the order from light to dark next, and black the least. This law was generalized from the results of fusion and limen experiments in a large number of meridians of the retina. Our second point was that the combination of the preëxposure and campimeter screen

most unfavorable to the saturation of the stimulus color was the most favorable condition for obtaining our phenomenon. The effect of preëxposure and campimeter screen upon the saturation of the stimulus color was measured in two ways: (1) by the effect on the limen color; and (2) by the effect on the limits of sensitivity.[1] Data were thus obtained which we could directly correlate with the frequency with which color was obtained in the after-image, and so determine whether our position was correctly taken. The results of this correlation show that, estimated in both these ways, the combination that proved the least favorable to the saturation of the stimulus gave color in the after-image in the largest percentage of cases; and, conversely, that the combination most favorable in its action on the stimulus gave color in the after-image in the smallest percentage of cases. A third point was that a black preëxposure and black screen gave the brightness conditions that were most unfavorable to the saturation of the stimulus color. An estimation of the effect of the different combinations of screen and preëxposure by either of the methods mentioned above brings out this point strongly. The least effect was found when preëxposure and screen were both of the brightness of the stimulus color. A fourth point was that preëxposure provides a stronger means of reducing the saturation of the stimulus color than does the screen. To make the test of this point absolute, the influence of one should be completely eliminated while the influence of the other is being determined. This can not be done. The eye must always have some preëxposure, and there will

[1] See Rand, G., 'The Factors Which Influence the Campimetrical Observation: A Quantitative Examination and Methods of Standardizing.' (In press.)

Of these two tests, the limen test has a much broader application, and measures much more directly what needs to be measured. It has a broader application because it can be made anywhere in the zone of sensitivity. It measures more directly what needs to be measured, because the results obtained show just how much color has to be present under a given condition to be sensed as color, while the results in the method of limits only express in terms of degrees how much the limit of sensitivity has been changed. This is a poor measure of how much the color in the stimulus has been inhibited under a given condition: because, in the first place, it is not a direct measure of this action; and, in the second place, the results obtained cannot be even roughly rendered into terms of direct measurement, owing to the fact that the sensitivity of the retina near the limits does not fall off either gradually or regularly.

always be a surrounding field. The effect of preëxposure, however, can be minimized by choosing it of the gray of the brightness of the color to be used as stimulus, and by making the stimulation to it extremely short. Working in this fashion, we have only a slight local brightness adaptation to modify the color excitation immediately following, the effect of which can be taken as practically negligible. The influence of the screen also can be minimized in a similar way, by having it always of the brightness of the color to be used as stimulus. Isolating the action of preëxposure and screen by this method, we estimated the effect of each in turn upon the saturation of the stimulus color, both by the effect on the limen of color and on the limits of sensitivity. Our results in both cases show that preëxposure can be made much the stronger factor. For example, the limits of sensitivity were never made to vary more than 4° by the most extreme changes that could be made in screens, while it could be varied as much as 14° by changes in the preëxposure. The difference stands out still more strongly in the effect on the limen, as would naturally be expected, since changes in the limen more directly express the differences in the inhibitive action than changes in the limits of sensitivity, as was shown in the footnote, p. 219. A fifth point was that black is the most favorable brightness quality for the projection field. This was shown very clearly by using projection fields of white, gray of the brightness of the stimulus color, and black, with each of the combinations of preëxposure and screen, and comparing the results obtained. In addition, these results, when compared with those obtained by varying the preëxposure and the campimeter screen, show that the brightness of the projection field is a very important factor—much more important than the brightness of the campimeter screen, and just as important, possibly more so than the brightness of the preëxposure.

Results in support of these points in general are given in Table I. In this table is shown the percentage of cases, based on twenty trials, in which color was obtained in the after-image when none was sensed in the stimulus. The object of the experiments was to determine the relative importance of the

three factors, preëxposure, campimeter screen, and projection field. The method of experimenting in the following cases was to keep two of the factors constant and find the effect of varying the third. However, in order to show most effectively the most favorable conditions in decreasing order, the results have been grouped as follows. If the individual, horizontal columns are compared, the effect of campimeter screen is shown. If groups of three are compared, the effect of preëxposure is seen. If compared in groups of nine, the effect of projection is seen.

From the table it will be seen that projection field and preëxposure are the most important factors. Of the preexposures, black is seen to be the most important. Its effect is greater on green and blue than on red and yellow. This is because the inhibitive action of the white after-image following the preëxposure is greater for blue and green than for red and yellow. The full effect of preëxposure was not obtained in our experiments because with a given black preëxposure we did not work as near the center of the retina as we might have done.

In method (*c*) also (see p. 208), the vertical campimeter was used to give the stimulus. Black was chosen both for the campimeter screen and for the projection screen in each case. Otherwise the procedure was the same as for after-images in direct vision. The results obtained were also similar, with the exception that not so much decrease of illumination was needed to obscure the stimulus color, and more saturated after-images were obtained. The stimulus time in indirect vision must always be shorter than in direct vision. (From 2 to 3 seconds was used.) This is due to the rapid exhaustion to color in indirect vision. After-images fall off in saturation if exhaustion to the stimulus color is carried beyond the stationary-point.

2. *Contrast*

It was found that contrast could be induced for certain colors when the general illumination was sufficiently reduced to obscure the color in the inducing stimulus. Very strong

TABLE I

SHOWING THE PERCENTAGE OF CASES IN WHICH THE COLORED AFTER-IMAGE WAS OBTAINED WHEN NO COLOR WAS SENSED IN THE STIMULUS, UNDER ALL VARIATIONS OF SCREEN, PREËXPOSURE, AND PROJECTION FIELD

Campimeter Screen	Preëxposure	Projection	Red	Yellow	Green	Blue
Black	Black	Black	100	100	100	100
Gray	Black	Black	100	100	80	70
White	Black	Black	50	40	50	40
Black	Gray	Black	40	30	30	20
Gray	Gray	Black	50	30	0	0
White	Gray	Black	50	40	0	0
Black	White	Black	50	30	40	0
Gray	White	Black	45	20	0	0
White	White	Black	45	30	0	0
Black	Black	Gray	40	70	50	40
Gray	Black	Gray	20	40	20	20
White	Black	Gray	20	30	0	0
Black	Gray	Gray	0	0	0	0
Gray	Gray	Gray	0	0	0	0
White	Gray	Gray	0	0	0	0
Black	White	Gray	0	10	0	0
Gray	White	Gray	0	0	0	0
White	White	Gray	0	10	0	0
Black	Black	White	0	0	10	0
Gray	Black	White	0	0	0	0
White	Black	White	0	20	0	0
Black	Gray	White	0	0	0	0
Gray	Gray	White	0	0	0	0
White	Gray	White	0	0	0	0
Black	White	White	0	0	0	0
Gray	White	White	0	0	0	0
White	White	White	0	0	0	0

contrast was aroused under these conditions by standard red, dark orange, and yellow of the Hering series of colored papers.

Contrast discs were cut as follows. The inducing surface was made of two colored discs; one 25 cm., the other 10.5 cm. in diameter. The contrast surface was made by placing between these discs on the color-mixer a black and white disc, 11.5 cm. in diameter. When these were rotated, the black and white mixed to give a gray ring 1 cm. in width, separating the two colored surfaces. The proportions of black and white were taken so that the gray ring matched in brightness the

color used, the brightness of the color having been determined by means of Schenck's flicker photometer.

The observations were made in the dark-room described above. The color of the inducing surface was obscured by a decrease of the illumination. As in the after-image experiments, this was done by two methods, one in which the observation was made after the eye had adapted to the illumination chosen; the other before adaptation had set in. The latter method gave a much stronger effect, for although decrease of illumination within wide limits increases contrast effect in general, this increase is very much greater while the illumination is decreasing. That is, if two determinations of contrast are made for the same inducing and contrast surfaces, one in which the judgment is passed while the illumination is decreasing, the other after the eye has become adapted to the illumination at which the former judgment was made, it will be found that the former determination greatly exceeds the latter. Thus it appears that color induction is greatly enhanced while the retinal change corresponding to dark-adaptation is going on. This phenomenon will be treated more fully in a later paper on contrast. Our present purpose is satisfied with the consideration of the phenomenon at one point in the series of decreasing illumination; namely, the point at which the color in the inducing surface is obscured. This point varies greatly for the different colors used. It is the highest for red, next highest for yellow and dark orange, lowest for green, and next lowest for blue.

The amount of contrast induced in each case was determined by two methods. In each method a measuring-disc was used, compounded from discs of the proper colors and of black and white. In the first method, the comparison-judgment between the contrast ring and the measuring-disc was made at the illumination at which the inducing color was obscured. This method may not be clear to the reader. It may seem, for example, that a degree of illumination that wholly obscures the inducing color would also wholly obscure the color of the measuring-disc. This is not true because of the different effect of the decrease of illumination upon the saturation of the dif-

ferent colors, *i. e.*, blue, green, and blue-green, the colors used for the measuring-discs, retain considerable color at the illumination at which red, yellow, and dark orange, the inducing colors used, lose their saturation. The difficulty with the method is, that the measurements on the comparison-disc at a low illumination are not of standard value because of decrease of saturation, and thus convey little meaning to the mind of the reader. In order to get measurements in standard terms, a second method was resorted to, the results of which are more intelligible although the method of judgment is less accurate. In this method the comparison was made in terms of the saturation of colors on the measuring-disc at full illumination. Since one of the terms of comparison is a memory-image, a time error was involved in this method. This was to some extent compensated for, however, by working both ways, *i. e.*, a part of the judgments were made by first getting a memory-image of the contrast sensation at decreased illumination and comparing that with the measuring-disc at full illumination, and a part were made by the inverse procedure.

The tables recorded in the report are compiled from the results of Misses Chamberlain (*C*) and Rand (*R*), fellows in psychology, of Bryn Mawr College, and Bunker (*B*), graduate student.

3. *The Purkinje-Brücke Phenomenon*

The Purkinje-Brücke phenomenon was found by us to demonstrate in a very striking fashion that it is not necessary for the inducing excitation to condition sensation directly in order that color induction may take place.

This phenomenon was first described by Purkinje in 1825.[1] He says: "Man liege ein weisses Quadrätchen von der Breite zweier Linien auf einen schwarzen Grund, starre es 20–30 Secunden an, und blicke sodann ins Schwarze hinein, so wird man ein noch dunkleres Viereck sehen, dessen Randes mit einem graulichen, sich allmählich verlierenden Scheine umgeben sind. Lagt man auf den schwarzen Grund statt des weissen Quadrätchens ein rothes, so zeigt sein grünes Spectrum einen

[1] Purkinje, J., 'Beobachtungen und Versuche zur Physiologie die Sinne,' 1825, II., p. 107.

TABLE II

C. SHOWING THE AMOUNT OF CONTRAST INDUCED WHEN THE COLOR IN THE INDUCING SURFACE IS UNSENSED

Stimulus	Contrast Ring	Direct Judgment	Memory-image Judgment
Red.....	White 41° Black 319°	Green 130° Black 230°	Green 171° Black 119° White 70°
Yellow.....	White 236° Black 124°	Blue 151° Black 130° White 79°	Blue 76° Black 223° White 61°
Dark orange.....	White 82° Black 278°	Green 210° Blue 56° Black 94°	Green 168° Blue 14° Black 172° White 6°

TABLE III

OBSERVER *B*

Stimulus	Contrast Ring	Direct Judgment	Memory-image Judgment
Red.....	White 41° Black 319°	Green 228° Black 132°	Green 214° Black 94° White 52°
Yellow.....	White 236° Black 124°	Blue 121° Black 142° White 97°	Blue 163° Black 59° White 138°
Dark orange.....	White 82° Black 278°	Green 136° Blue 118° Black 106°	Green 152° Blue 106° Black 102°

TABLE IV

OBSERVER *R*

Stimulus	Contrast Ring	Direct Judgment	Memory-image Judgment
Red.....	White 41° Black 319°	Green 225° Black 135°	Green 166° Black 126° White 68°
Yellow.....	White 236° Black 124°	Blue 137° Black 189° White 34°	Blue 82° Black 159° White 119°
Dark orange.....	White 82° Black 278°	Green 195° Blue 64° Black 52° White 49°	Green 127° Blue 44° Black 159° White 30°

rothlichen Schein; auf gleiche Weise das blaue Spectrum einen orangen Schein, u.s.w. Man sieht heraus dass die objective Farbe nicht bloss in die Tiefe der Retina, sondern auch in die Breite einwirkt jedoch nicht gleich mässig nach ihrer ganzen Ausbreitung, sondern zunächst an der Gränze der heterogenen Beleuchtungen am intensivesten."

The phenomenon with some modifications was next described by Brücke in 1851,[1] and by Aubert in 1862.[2] The color or added brightness in the after-image was called by Brücke an after-effect of induction;[3] by Aubert an after-image of contrast. Attention was again called to the phenomenon by Hering in

[1] Brücke (*Pogg. Ann.*, 1851, LXXXIV., pp. 418–448) says (p. 47): "Einen dritten Beweis endlich kaum man aus der Beobachtung der negativen Nachbilder entnehmen, welche nach diesem Versuchen zur Erscheinung kommen welche zeigen, dass die inducirten Farben als solche im Stande sind, complementären gefärbte Nachbilder hervorzurufen." His demonstration consists in getting the after-effect of looking towards the light through squares of red, green and violet glass with small discs of black paper placed at their centers. He describes the after-effect as follows: "Bei Anwendung des rothen Glases erscheint als negatives Nachbild eine helle rothe Scheibe auf dunkel grünem Grunde. Hierein liegt nichts Auffallendes und dieser Erfolg würde sich nach Analogie der Versuche von Fechner erklären lassen, auch ohne dass man eine Nachwirkung der inducirten Farbe voraussetze. Wende ich aber das grüne Glas an, so habe ich von der dunklen Scheibe ebenfalls ein helles rothes Nachbild und der Grund ist Schwarz, oder wenigstens so dunkel, dass ich seine Farbe nicht mit Sicherkeit habe unterscheiden können. Hier hat also das inducirte Grün Roth hervorgebracht. während das inducirende gleichzeitig kein deutlich gefärbtes Nachbild erzeugte, In derselben Weise zeigte sich mir bei Anwendung des violetten Glases das negative Nachbild als eine gelbgrüne Scheibe auf schwarzem Grunde." In two cases here Brücke apparently has the positive excitation induced across the black disc, and in two cases the negative excitation. But since he does not make induction apply to the negative excitation, as has been done later, he does not explain this case as after-image of contrast, but makes it instead analogous to the phenomenon described by Fechner.

[2] Aubert (*Pogg. Ann.*, 1862, CXVI., pp. 249–279) says on p. 259: "Ausserdem ist es aber auffallend dass der simultane Contrast selbst noch einen successiven Contrast hervorruft, indem die durch den simultanen Contrast complementär gefärhten weissen Quadrate noch einmal complementäre Nachbilder hervorrufen, so dass jene Quadrate im Nachbilde dieselbe Farbe, nur sehr bedeutend abgeschwächt haben wie der Grund." Aubert observed white squares on a colored ground. In the positive sensation the squares took on a tinge of color complementary to the background, and in the after-image of the same color as the background. That is, a white square on a red background appeared greenish in the stimulus and red in the after-image. He says: "Besonders schön und mit einem eigenthümlichen Glanze erschienen die Nachbilder der weissen Quadrate auf dem blauen Streifen."

[3] Brücke applies the term induction to the positive excitation only.

1878.[1] Hering worked with achromatic sensation alone. He also called the phenomenon an after-image of contrast sensation,[2] and upon his observations based his arguments against Helmholtz's theory of contrast. Continuing the discussion, the experiments were extended to color sensations by Ebbinghaus[3] who considered the phenomenon a combination both of after-image of contrast and of contrast induced by an after-image.

The following form of experiment was adapted by us from Ebbinghaus. Squares 20×20 cm. of red, green, blue, and yellow Hering papers were fastened upon neutral gray backgrounds. Passing vertically through the center of these squares, gray strips 2×20 cm. of Hering papers numbers 8, 24, 2, 41, respectively, were pasted. The after-effect of stimulation by this combination is a square of a color complementary to the color of the stimulus square, traversed by a strip of the same color as the stimulus square. The red square, for example, gives a green square traversed by a strongly saturated

[1] Hering, E., 'Zur Lehre vom Lichtsinn,' Wien, 1878, pp. 5–18.

[2] On pp. 5–18 Hering describes experimental conditions similar to those described by Purkinje. Here he calls the phenomenon successive light induction. On pp. 24–29 he describes slightly different experimental conditions. Two dark gray strips of the same brightness, 3 to 4 cm. long and .5 cm. broad, are fastened parallel to each other 2 cm. apart, one upon a white and the other upon a black ground. A fixation-point is taken on the boundary between the white and black backgrounds midway between the strips. By contrast the strip on the white ground looks darker than the strip on the black ground. When an after-image of the strips, the one lightened, the other darkened by contrast, is obtained, their brightness values are reversed, and a still greater brightness difference between them is found. This difference toward black on one hand, and toward white on the other, Hering calls an after-image effect of the brightness contrast induced in the stimulus. The phenomena obtained by these two sets of conditions are in every sense identical. One can scarcely see a reason for the separate treatment they have received by Hering.

[3] Ebbinghaus (*Grundzüge der Psychologie*, Erster Band, p. 239) says: "Man lege zwei mässig grosse Blätter z. B. von sattgrüner Farbe so auf einen grauen Grund, dass nur ein schmaler, etwa 5 mm. breiter horizontaler Streifen zwischen ihnen freibleibt, und lasse diesen von einer unbefangenen Person eine Weile fixieren. Dann lasse man sie das Nachbild auf einem etwas unregelmässig geformten Grunde entwerfen, z. B. auf dem Fensterkreus, und frage, was sie sehe. Man wird so gut wie ausnahmlos die Antwort erhalten: 'einem grünen Streifen.' Der objektiv völlig neutrale Streifen hat durch die zweimalige Kontrastwirkung (im Vor- und Nachbilde), die sich von einer ausgedehnten Umgebung auf seine schmale Fläche konzentriert, eine so intensive Färbung bekommen, dass er sofort die Aufmerksamkeit auf sich zieht, während die röthliche Nachbildfärbung seiner Nachbarschaft bei den Unregelmässigkeiten der reagierenden Fläche in der Regel gar nicht beachtet wird."

red strip; the green square, a red square traversed by a green strip. Now if the color of the strip is an after-effect of a previously induced contrast, we have a strongly saturated, long-enduring after-image of an unsensed stimulus, for the brightness opposition of the gray strip to the inducing color, and the rather intensive illumination under which we worked, both combined to inhibit all contrast color in the stimulus. At this stage of the work we do not feel prepared to take positive ground on the question of explanation, but have the following evidence to offer that the Brücke interpretation is correct.

i. Evidence that the Color in the Strip is an After-Image of a Previous Contrast Sensation, rather than Contrast in the After-Image

(*a*) In the after-effect, the strip and square apparently develop, fluctuate, and die away independently of each other; the strip frequently develops before the square, especially if the stimulation has been very short; it invariably lasts longer than the square, returning several times after the square has finally disappeared; and in fluctuating, the two figures behave much as two after-images are observed to do, so far removed from each other as to be wholly without the sphere of reciprocal influence. The strip is frequently present when the square has disappeared, and vice versa. It rarely happens that their phases coincide, and when they do, the connection is obviously a chance one.

Records on this and the following points were taken from a number of observers, both experienced and inexperienced. The results given in the following tables are typical. The work was done in a large optics room, lighted on one side by a bank of windows extending nearly to the ceiling. The observer, head in rest, was seated in front of these windows so that the light coming from above and either side fell uniformly upon the projection-field of engine-gray cardboard, 1 meter distant. The time of stimulation, unless otherwise stated, was 30 seconds, and the unit of record was one second. The recording apparatus used throughout consisted of a Ludwig-Baltzar kymograph; a double electro-magnetic recorder, and two con-

tact keys, one for strip and one for square; a Jacquet chronograph set to seconds; and a lamp rheostat to reduce the current from the lighting circuit.

TABLE V

C. SHOWING THE INDEPENDENT PHASES OF VISIBILITY AND INVISIBILITY OF SQUARE AND STRIP

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 cm. . . .	8	5	6.2	56	7.1	57	113
Gray No. 8, 2×20. .	15	6	5	89.5	4	60	149.5
Green, 20×20	2	6	5	15	3.8	7.5	22.5
Gray No. 24, 2×20.	6	7	4.6	32	3.7	22	54
Yellow, 20×20.	3	17	8	32	11	33	65
Gray No. 41, 2×20.	8	18	6.4	58	2.4	19	77
Blue, 20×20.	4	12	6.5	32.5	5	20	52.5
Gray No. 2, 2×20. .	9	11	6.1	61	3.7	33	94

TABLE VI

OBSERVER *B*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 cm. . . .	11	12	8.5	102	4	44.5	146.5
Gray No. 8, 2×20. .	18	9.5	4.2	79	4	72	151
Green, 20×20.	4	5	3.1	15.5	4.1	16.5	32
Gray No. 24, 2×20.	6	7	4.4	30.5	3.8	22.5	53
Yellow, 20×20.	3	3.5	3.1	12.5	7.5	22.5	35
Gray No. 41, 2×20.	7	3	3.1	25	3.3	23	48
Blue, 20×20.	6	13	4.1	28.5	7.3	44	72.5
Gray No. 2, 2×20. .	11	3	3.6	43	3.3	36	79

TABLE VII

OBSERVER *R*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 cm. . . .	3	5	4.8	19	10.6	32	51
Gray No. 8, 2×20. .	7	5	3.5	28	3.3	23	51
Green, 20×20.	3	2	6.2	25	4	12	37
Gray No. 14, 2×20.	5	7	5.3	32	3.6	18	50
Yellow, 20×20.	2	12	7.7	23	9.5	19	42
Gray No. 41, 2×20.	7	13	5.5	44	2.3	16	60
Blue, 20×20.	3	22	9.8	39	4.3	13	52
Gray No. 2, 2×20. .	10	7	4.2	46	3.1	31	77

(*b*) The mutual independence of strip and square can be further indicated by a method of concomitant variations. That is, the strip can be made to fluctuate less and last longer without any corresponding change in the fluctuation and duration of the square. And conversely, the square, or rather what in this variation corresponds to it, can be made to change its duration and rate of fluctuation, without any corresponding change in the fluctuation and duration of the strip. Both of these variations are based upon the effect of the arrangement relative to the direction of greatest involuntary eye-movement, upon the fluctuation and duration of a strip after-image. If the observer has more eye-movement in the horizontal than in the vertical, a strip after-image with its greater dimension in the vertical will fluctuate more and last a shorter time than one of the inverse arrangement. It may be stated as a law[1] that whenever the direction of greatest eye-movement is along the shorter dimension of the after-image, the maximal fluctuation and minimal duration is attained for that form of after-image.

Thus, to vary the duration and rate of fluctuation of the strip without changing them in the square, we need only to arrange the stimulus so that the longer dimension of the strip is first in the vertical and then in the horizontal. This rotation of the stimulus 90° will obviously have no effect upon the duration and rate of fluctuation of the after-image of the square, since both of its dimensions are equal. If we wish to make the converse variation, *i. e.*, change the duration and rate of fluctuation of the outer figure without changing them for the inner, we shall obviously have to make the outer figure a strip and the inner a small square. Then by rotating the stimulus 90° we shall increase or decrease the duration and rate of fluctuation of the outer figure, depending upon whether its shorter dimension is in the vertical or horizontal, while the duration and rate of fluctuation for the inner figure will not be affected.

(*c*) The Brücke interpretation seems also to receive negative support from the following fact. When one observes the

[1] Ferree, C. E., 'Intermittence of Minimal Visual Sensations,' *Amer. Journ. of Psychol.*, 1908, XIX., pp. 101–103. For explanation of this phenomenon see same reference, pp. 126–127.

TABLE VIII

C. SHOWING BY METHOD OF CONCOMITANT VARIATIONS A DECREASE IN FLUCTUATION AND AN INCREASE IN DURATION OF THE STRIP WITH NO SIGNIFICANT CHANGE IN THE PHASES OF THE SQUARE

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×20 cm.	3	25	15.3	61	5.7	17	78
Gray No. 2, 2×20 (vertical)	11	12	6.8	81	3.3	36	117
Blue, 20×20.....	2	48	20.6	62	5.8	11.5	73.5
Gray No. 2, 2×20 (horizontal)	9	11	9.4	94	4.3	39	133

TABLE IX

OBSERVER *B*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×20	7	6	6.2	50	3.9	27	77
Gray No. 2, 2×20 (vertical)	13	4	4.1	57	2.2	29	86
Blue, 20×20	10	13	4.4	48	2.4	24	72
Gray No. 2, 2×20 (horizontal)	9	5	7	70	2.9	26	96

TABLE X

OBSERVER *R*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×20	3	17	7.5	30	5	15	45
Gray No. 2, 2×20 (vertical)	7	7	4.4	35	2.1	15	50
Blue, 20×20	3	14	7.8	31	3.5	14	45
Gray No. 2, 2×20 (horizontal)	3	14	10.5	42	3.3	10	52

TABLE XI

C. SHOWING BY METHOD OF CONCOMITANT VARIATIONS A DECREASE IN FLUCTUATION AND AN INCREASE IN THE DURATION OF THE SQUARE WITH NO SIGNIFICANT CHANGE IN THE PHASES OF STRIP

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×.5 (vertical)	9	9	4.3	43	1.5	14	57
Gray No. 2, .5×.5 .	0	9	9	9			9
Blue, 20×.5 (horizontal)	5	15	9.7	58	2.4	12	70
Gray No. 2, .5×.5 .	0	7	7	7			7

TABLE XII

OBSERVER *B*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×.5 (vertical)	8	6	3.3	30	2.3	18	48
Gray No. 2, .5×.5 .	0	7	7	7			7
Blue, 20×.5.....	4	10	9	45	2.5	10	55
Gray No. 2, .5×.5 .	0	8	8	8			8

TABLE XIII

OBSERVER *R*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×.5.....	7	5.5	4	32	2	14	46
Gray No. 2, .5×.5 .	0	5	5	5			5
Blue, 20×.5 (horizontal)	3	43	17	68	1	3	71
Gray No. 2, .5×.5 .	0	10	10	10			10

stimulus through tissue-paper or under decreased illumination, the relative duration of strip to square in the after-effect is very greatly increased. This should not take place if a color in the strip after-image is induced by the color in the after-image square. The ratio should remain constant or approximately so, *i. e.*, since the intensity of the after-image square has been reduced by weakening the color of the stimulus square by tissue paper, a corresponding weakening would be expected in the color induced in the strip. The observation thus seems to furnish a negative indication that the after-image interpretation is correct. Relative to this observation, however, two points need to be taken into account. (1) An inspection of the tables shows that although there is an increase in the ratio of duration of strip to square, there is an actual decrease in the absolute duration of the strip. This might be supposed to indicate a tendency for the strip to vary with the square and thus to favor the contrast interpretation; but the increase of relative duration is too great for this to be probable. Besides, the decrease in absolute duration can be readily accounted for from the other side by a decreased retinal induction in the

stimulus due to the decreased saturation of the square. (2) The second point is quite aside from differential evidence as between the Brücke and Ebbinghaus interpretations. When the stimulus was observed through tissue-paper or under decreased illumination, considerable contrast color developed in the stimulus, where before there had been none, yet the after-image was less than in the former case. This seems to indicate the following. (*a*) The contrast sensation is only an equivocal index of the amount of excitation set up on the retina by a neighboring surface. This excitation may under one set of conditions arouse an intensive sensation, and under other conditions be equally strong, at least as far as after-effect goes, and excite no sensation. (*b*) Brightness opposition inhibits only the contrast sensation. It apparently does not inhibit the corresponding retinal induction due to the neighboring surface, at least not its power to give after-effects. That is, the brightness opposition between the square and strip in the stimulus was greater when the tissue-paper was not used and yet the strongest after-image of the contrast excitation was obtained in this case. These suggestions are thrown out merely tentatively and are meant to apply only within the bounds of the evidence offered.

Since the results are similar for both the tissue-paper device and the decrease of illumination, tables will be given only for the former.

TABLE XIV

C. SHOWING THE PHASES OF INVISIBILITY AND VISIBILITY OF STRIP AND SQUARE AND THEIR RELATIVE DURATIONS, OBSERVED UNDER TISSUE PAPER

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20	0	3	3	3			3
Gray No. 8, 2×20	3	4	2.5	10	3	9	19
Green, 20×20	0	7	7	7			7
Gray No. 24, 2×20	2	5	3.3	10	6.5	13	23
Yellow, 20×20	1	8	4.8	9.5	3	3	12.5
Gray No. 41, 2×20	4	8	3.8	19	3.6	14.5	33.5
Blue, 20×20	1	5	6	12	6	6	18
Gray No. 2, 2×20	4	3	2.3	14	2.5	10	24

TABLE XV

OBSERVER *B*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20.....	0	2	2	2			2
Gray No. 8, 2×20..	3	1	1.9	7.5	4.8	14.5	22
Green, 20×20	0	7	7	7			7
Gray No. 14, 2×20.	2	4	4	12	5.8	11.5	23.5
Yellow, 20×20	0	4.5	4.5	4.5			4.5
Gray No. 41, 2×20.	3	4	3.1	12.5	4	12	24.5
Blue, 20×20.....	1	3	2.5	5	7	7	12
Gray No. 2, 2×20 .	7	4	3.5	28	3.1	22	50

TABLE XVI

OBSERVER *R*

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20.....	0	3	3	3			3
Gray No. 8, 2×20..	3	3	2	8	3.3	10	18
Green, 20×20	0	8	8	8			8
Gray No. 14, 2×20.	3	4	3.5	14	3.3	10	24
Yellow, 20×20	1	6	4.5	9	2	2	11
Gray No. 41, 2×20.	3	5	4.5	18	4	12	30
Blue, 20×20	1	5	4	8	5	5	13
Gray No. 32, 2×20.	4	5	4.6	23	3	12	35

ii. But if a Contrast Effect, Evidence that it May Take Place when the Inducing Color is Unsensed

As already stated, the strip frequently develops before the square; it invariably lasts longer than the square; returning several times after the square has finally disappeared; and in fluctuating its phases rarely coincide with those of the square, *i. e.*, it is frequently visible when the square is invisible, and conversely.

Thus it is immaterial for our thesis which interpretation be given to the phenomenon. For (*a*) if the after-color in the strip be a contrast sensation, our results show that it may be set up when the inducing excitation is not directly conditioning sensation; and (*b*) if it be an after-image sensation, they show that it may be aroused by a previous excitation which did not itself directly give rise to sensation.

FIG. 1. (Observer *R.*) Records showing the fluctuations and duration of strip and square.

III. Explanation

A later paper will contain a history of the observations on the effect of brightness changes, general and local, upon color phenomena, beginning with Purkinje.[1] Purkinje's observations cover the following points. (1) The relative difference in the brightness values of the spectral colors at full and decreased illumination. (2) The difference in the effect of change in brightness upon the saturation of colors. (3) The changes in color-tone produced by changes in brightness.

After-Image

The explanation of the possibility of getting color as an after-image from a stimulus in which no color can be sensed rests in general with the second point of Purkinje's observations; namely, the difference in the effect of change in the brightness of different colors upon their saturation. In every case in which a colored after-image was obtained from a colorless stimulus, it was gotten at a degree of brightness which worked against color saturation in the stimulus and relatively favored it in the after-image. Take, for example, the case of central vision. By the first method the influence of the brightness factor was introduced by means of a decrease in the general illumination. Yellow and red and orange lose their saturation at an illumination that permits of a supraliminal saturation of blue and green. The case, then, is simple. There was retinal excitation in the cases of red, yellow, and orange, but it was obscured for sensation by the brightness factor. The after-effect of this excitation, however, was not obscured for sensation. It was relatively favored, and, therefore, gave the after-images which were observed. There is nothing new or strange in principle about this phenomenon. The foregoing results show that it depends entirely upon the effect of the brightness changes in a color upon its saturation. These effects were observed and reported as far back as the time of Purkinje, and have been discussed sporadically in the literature from that time to this. By the second method the unfavorable

[1] Purkinje, 'Beobachtungen und Versuche zur Physiologie der Sinne,' 1823, I.' p. 109.

brightness quality was added to the stimulus color by objective mixing, contrast, or after-image; and the favorable brightness quality was added to the after-image by means of the projection field. The after-image method was found to be the most effective for adding the unfavorable brightness quality to the colored stimulus because by means of it (*a*) the amount of colored light coming to the eye is not reduced as it is by the method of objective mixing; and (*b*) the unfavorable brightness quality added to the stimulus color is not added to the after-image color also, as is done under the conditions of the experiment by the method of contrast.

For the after-image in peripheral vision, we have a slightly different case. The peripheral retina differs from the central with regard to the effect of change in brightness upon both the saturation and the quality of colors. With regard to saturation, we have in general merely an exaggeration of the condition found in the central retina, *i. e.*, brightness changes produce greater difference in effect in the case of the different colors. With regard to color tone, the change is not in the same direction in every case as it is in central vision, *i. e.*, in central vision at full illumination. In this paper, however, we are concerned with the effects upon saturation alone. Moreover, under a given set of conditions more of the brightness quality can be added as after-image and contrast in this region of the peripheral retina than in the central retina because of the increased sensitivity of the former to achromatic after-image and contrast. We have then in these regards especially favorable conditions in the peripheral retina for obscuring stimulus color by brightness changes, and for relatively favoring the development of the after-image. We have as an additional factor the enhanced sensitivity of the peripheral retina to adaptation and after-image effects. It adapts very rapidly to color stimuli and responds quickly with a vivid after-image of short almost momentary duration, often described as a vivid flash of color. All of these factors make it comparatively easy to get after-images of unsensed stimuli in peripheral vision, the only fact relative to our problem that needs to be explained.

Contrast

It was found to be especially easy to arouse green, blue-green, and blue as contrast sensations when their inducing stimuli do not directly excite a sensation of color. Two factors are involved in this result. (1) A decrease of the illumination obscures red, orange, and yellow before it obscures their contrast colors, green, blue-green, and blue. Hence, induction and the effect of decrease of illumination upon it aside, there is reason in the nature of the color processes themselves why red, for example, should not be sensed when its contrast color, green, is sensed. But (2), in addition to this, decrease of illumination enormously enhances the induction of the contrast color, this effect being greater for green, blue, and blue-green than for their complementary colors red, yellow, and orange.

The Purkinje-Brücke Phenomenon

Until a final decision has been made between the after-image and the contrast interpretations, the Purkinje-Brücke phenomenon presents a two-fold problem. From the side of the after-image interpretation it must be explained how an after-effect so intensive and of such long duration can be obtained from a stimulus of so little apparent intensity; *e. g.*, in our form of the experiment, no color at all could be sensed in the strip. From the side of the contrast interpretation, it must be shown how contrast color can be gotten in the after-effect of the strip when the square is not in sensation; *i. e.*, before the square appears, in the invisibility phase of its intermittence, and after its final disappearance.

At this stage of the work, an explanation will not be attempted. From the Brücke side, however, it may be pointed out that evidence has already been given that color in the contrast sensation is only an equivocal index of the actual amount of the corresponding retinal color excitation. Also, that, while this induced excitation may arouse a strong sensation, a weak sensation, or even none at all, depending upon concomitant brightness conditions, contour, etc., nevertheless, in all of these cases, it gives rise to after-sensations of color, and to the most intensive in the cases we used, when no color was sensed in the

stimulus. In fact, it was just to take advantage of this point that our method was devised. We introduced brightness opposition between strip and inducing color to prevent the induced excitation from arousing a sensation of color, knowing that its power to condition color in the after-image was not diminished thereby, providing the brightness conditions were favorable for the development of the color. The brightness conditions were made tolerably favorable by choosing a shade of gray for the stimulus strip whose after-image was approximately the brightness of the after-image color. From the side of the Ebbinghaus interpretation, the writers have not at present even a plausible suggestion to offer in explanation. However, lack of explanation of this phenomenon does not limit its confirmation of our thesis that stimuli in which no color is sensed may arouse after-image and contrast sensations in which color is sensed.

We wish to state in conclusion that our chief interest in the problem has been its share in the broader problem presented by the Purkinje observations. We do not think that sufficient attention has been given to the second and third of these observations and the light they may throw on color theory. Up to this time, theory has practically ignored the effect of brightness changes upon the saturation and quality of color sensation. These effects seem to argue a functional connection between chromatic and achromatic processes which should not be disregarded.

A NEW LABORATORY PENDULUM[1]

BY KNIGHT DUNLAP

Having need of a large pendulum for various sorts of work, and finding none of the instruments in the market exactly suited to our purposes, we have had constructed the instrument represented schematically in Figs. 1 and 2. It was built in the workshop of the Johns Hopkins physical laboratory by Mr. Childs, has been in daily use for nearly a year, and has proved very satisfactory. Figs. 1 and 2 are approximately 1/12 diameter.

The essential framework is composed of hot-rolled bar steel, with a cast iron headpiece (*S*). The head-piece is a bottomless box with sides (Fig. 1) 14 × 5 inches, and ends (Fig. 2) 12 × 5 inches, with walls 11/16 of an inch thick. At the top, along each side, is a flange extending inwardly ⅝ of an inch; giving therefore two top surfaces each $1\frac{5}{16}$ inches wide, which are accurately planed for the seating of the knife rests, and for other attachments which are to be added later.

The base pieces (Z, Z), are 1 × 2½ inches in cross section, and 24 inches long. At each end they are provided with levelling screws (not shown). These levelling screws are lag screws, ¾ × 3¾ inches. The four oblique pieces (*V*, *V*) are 1 × ¾ inches in cross section, 52 inches long. These are fastened to the head piece (*S*) with two ½ inch lag screws in each, and are bolted to the horizontal piece (*X*, *X*) with ½ inch bolts. The horizontal pieces, (*X*, *X*), in turn are fastened to the base pieces (Z, Z) with two ⅜ inch lag screws in each end.

The frame as described is very rigid, and not excessively heavy. Two men can easily move it.

The pendulum rod (*R*) is of ⅝ inch round steel, 48 inches long. There are two bobs (*P*), of cast iron, one weighing about 25 lbs. and the other about 10 lbs. The lighter is used

[1] From the psychological laboratory of the Johns Hopkins University.

in reaction-time work, in which only a single swing of the pendulum is necessary. The bobs are circular in horizontal cross section, and of such shape in the vertical section (very

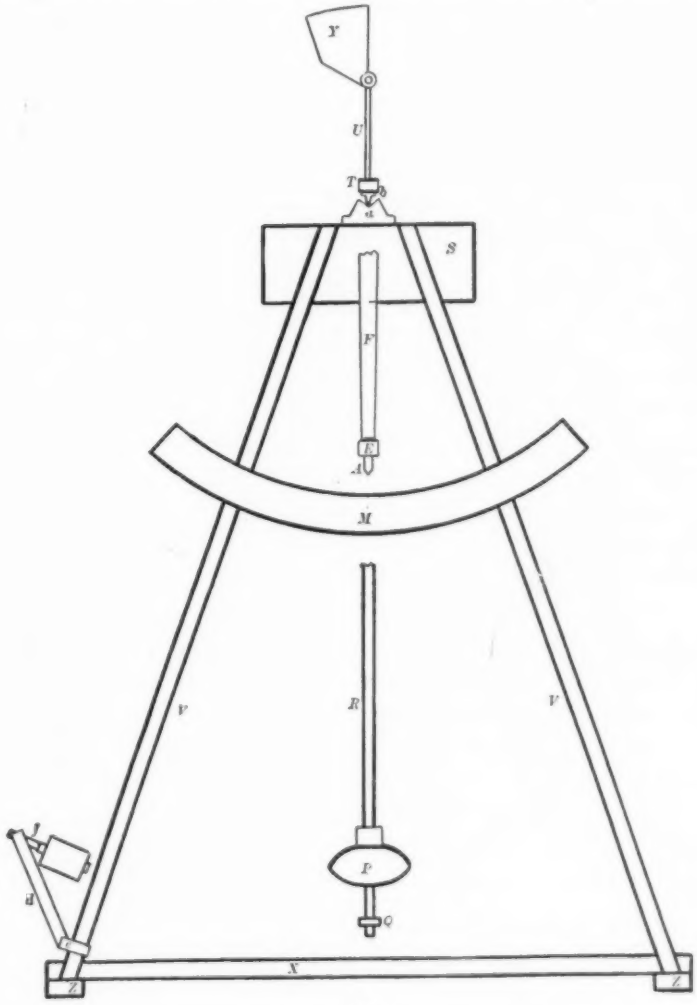

Fig. 1

rudely represented in the figures) that low resistance is offered by the air. The bob is fastened to the rod by means of a set screw (with square head for wrench) and is screwed into the rocking bar (*T*) and locked by a nut. The bar (*T*) is 1×¾

inch in cross section, 14 inches long; on it are mounted the knives (*b*, *b*), which are of tool steel, planed accurately and case hardened, as are also the rests (*a*, *a*). Lag screws are used to fasten the knives to the bar (*T*), and the rests to the headpiece (*S*).

Near the end of the rod (*R*) is mounted (adjustably) an armature (*Q*) of soft iron $\frac{1}{2} \times 1\frac{1}{2} \times 2\frac{11}{16}$ inches, by which the electro-magnet (*c*) holds the pendulum deflected. This magnet, by means of the clamp (*e*), the adjustable swing (*d*), and the rod (*f*) which slides through a clamp, is adjustable to hold the pendulum at any point up to thirty degrees from the center. As the knives are planed to thirty degrees, and the rests to ninety degrees, this is the maximal swing.

The arc (*M*) is of cast iron, dressed on a lathe to approximately $2\frac{1}{2} \times \frac{3}{8}$ inch in radial cross section, bolted to two clamps adjustable on the oblique pieces (*V*, *V*, Fig. 1), so that it may be accurately centered on the knife edges. In Fig. 2 is shown the projection of the vertical radial cross section of the arc. On this arc (*M*) are supported the contact devices, which are not represented in Figs. 1 and 2, but are shown at approximately $\frac{1}{2}$ diameter in Figs. 3 and 4.

Each contact device involves essentially a small permanent bar magnet (*G*), held in a brass pinion working in cone bearings. The lower end of the magnet (*G*) has platinized contact surfaces, working against the platinized tips of a contact screw on each side. One of these screws (*r*) is indicated in Figs. 3. These screws may both be of untempered steel, in which case the end of the magnet (*G*) will adhere to either when moved to it; or one may be of steel and the other of brass, in which case the magnet when released will always return to the steel screw. The strips (*H*, *J*) which carry the contact screws are insulated from the Y-piece (*O*) which supports the bearing of the magnet (*G*), and are provided with binding screws (*s*, *t*) through which connection may be made with either. A binding screw (*K*) in (*O*) provides for the other terminal. The current passes through the bearing of the magnet (*G*), but if this is not oiled and is kept clean, there is no trouble. It would, however, be better for general purpose

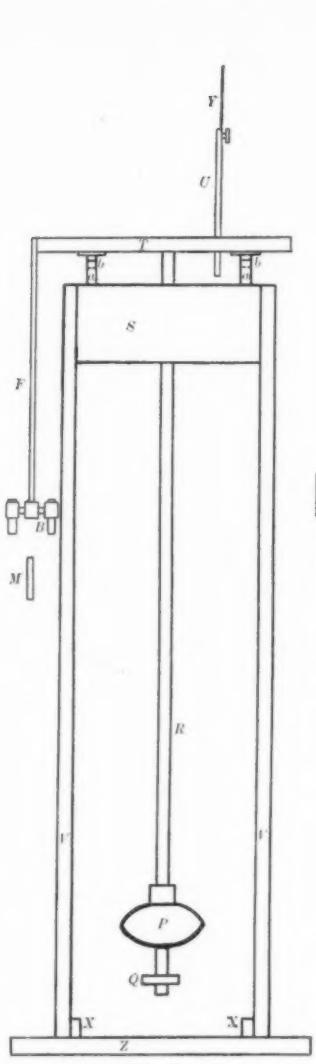

Fig. 2

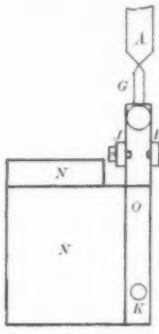

Fig. 4

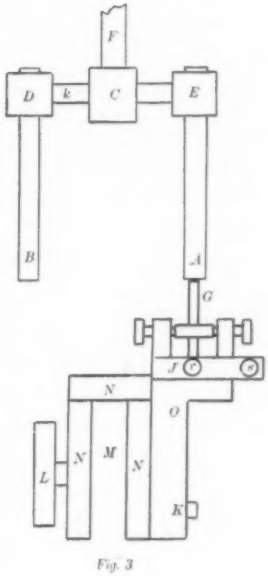

Fig. 3

to connect the pinion of (*G*) to (*O*) by means of a flexible conductor.

The small magnet (*G*) is moved by a master bar-magnet (*A*) carried by the arm (*F*) which is attached to one end of the rocker-bar (*T*). This magnet (*A*) is adjustable in the sup-

porting clamp (*E*), which is adjustable on the rod (*h*). Thus, the magnet (*A*), carried by the arm (*F*), may be so adjusted that it comes close to the magnet (*G*) without touching it. The upper end of (*G*) and the lower end of (*A*) are beveled at the apposed ends to a width of 1/32 inch.

The arm (*F*) carries another magnet (*B*) similar to (*A*). By reversing the contact device so that the clamp screw (*L*), is on the right, instead of on the left, the magnet lever (*G*) is brought under the master magnet (*B*). The contact devices are accordingly all made alike, and owing to the relations of the parts (*N*) and (*O*), two of them may be so adjusted that the contacts are simultaneously operated. The contact devices, except for the magnet (*G*) and the contact screws mentioned, are entirely of brass; as is also the arm (*F*) and the clamps which hold the large magnets (*A*) and (*B*). The parts (*C*, *k*, *h*) are planed and turned from one piece.

For certain exposure work, a screen is attached above the pendulum. The rod (*N*) is arranged to slide through a hole in the rocker bar (*T*) and to be fastened at the desired height by a set screw. The upper end of the rod carries a clamp in which can be secured a screen (*Y*) of aluminum, rubber, cardboard, or other suitable material, having properly cut apertures or edges. With the arrangement shown in Figs. 1 and 2, the screen exposes a light when the pendulum reaches a certain prearranged point in its swing from the magnet (*c*), and recovers it at the same point in its return. Exposures can be arranged below the bearings, by using a different type of pendulum rod.

It is probable that we will modify the instrument by placing the arc (*M*) inside the frame, instead of outside, as at present; and attaching the master magnets by a clamp directly to the pendulum rod at the proper height, doing away with the arm (*F*). The arm was attached because we intended to use a different type of contact, but found that it was too noisy. We planned to use electro-magnets to actuate the described contact devices, but have found the permanent magnets satisfactory for present purposes. These are readily kept at full strength by applying a powerful electro-magnet from time to time; not necessarily every day, although this is preferable as a routine matter.

A further improvement in the contact devices consists in the use of brass contact screws only, and the placing of the steel screws (which may be hardened in this case) just above these. The magnet-lever in this case need not touch the steel screw, which can be adjusted to give just the amount of pull required.[1] The simpler device has however worked well so far. These contacts are far superior to any others with which I am familiar; the reduction of noise effected by avoiding the striking of the pendulum attachment on the contact device is in itself of the highest importance.

Some tests have been made on the capacity of the instrument, but as it has been in constant use in a reaction-time experiment these tests have not been exhaustive. With the 25 lb. weight, the pendulum will swing for several hours after being released (the total time depending on the initial amplitude). If started with nearly its full amplitude, and a period of about 2 seconds (*i. e.*, 2 seconds to a complete swing: commonly called a period of 1 second), the period shortens by nearly fifteen sigma at the end of three hours. The greater part of this change occurs in the first hour, so that by starting with a smaller deflection, it is possible to obtain less than two sigma change (1/10 of 1 per cent.) in a half hour. In the reaction time work, since the pendulum is recaught by the magnet at the end of each swing, all swings are under the same conditions, and exact accuracy is obtained.

Full tests on the pendulum will be made as soon as it is released from the present experiment, and the detailed results will be reported in connection with another piece of work.

[1] This type of contact we have found necessary on the Schumann time-machine, on which it works admirably; even at rapid rates of rotation, when none of the ordinary forms of contact devices work at all.

DISCUSSION

CAN BIOLOGY AND PHYSIOLOGY DISPENSE WITH CONSCIOUSNESS?

"While the comparative psychologists debate concerning the amount of sensation, memory, reflection, that one should attribute . . . to animals, there (arises) in the growing science of comparative physiology an enemy to the death, of all comparative psychology."[1] So writes J. von Uexkuell, careful worker for the last twenty years on the physiology of animal behavior. The strife between the two sciences can only end, he adds, 'in the complete annihilation of one of the two combatants' for, "before objective investigation, the sensations, the memory and thoughts of animals disappeared like fluttering forms of vapor. The iron chain of objective changes, which began with the stimulation of the sense organ and finished with the movement of the muscle, was welded together in the middle. Nowhere remained a smallest spot for the psyche of the animal. Basing itself on these incontestable facts comparative physiology pronounced the psychological conclusions mere superstitions, and denied comparative psychology the right to call itself a science."[2]

A. Bethe, the eminent neurologist and student of animal behavior, with Beer, Ziegler, Nuel and others, is of the same opinion. Conscious states *may* exist but it is not probable; for scientific procedure they are then to be denied. "Chemico-physical processes and their consequences, that is the objective aspect of psychic phenomena, and these alone, should be the object of scientific investigation."[3]

J. Loeb, in his most recent article, writes: "The contents of life . . . are wishes and hopes, efforts and struggles . . . disappointments and suffering. And this inner life should be amenable to a physico-chemical analysis? In spite of the gap which separates us today from such an aim, I believe that it is attainable."[4]

[1] 'Psychologie u. Biologie in ihrer Stellung zur Tierseele. Asher u. Spiro: Ergebnisse der Physiologie,' I. Jahrg., II. Abth., V., p. 213; tr. by H. S. Jennings.

[2] *Ibid.*

[3] 'Die anatomische Elemente des Nervensystem u. ihre physiologische Bedeutung,' *Biol. Cent.*, Bd. 18, p. 864.

[4] *Pop. Sci. Mo.*, Jan., 1912, p. 19.

Other naturalists and physiologists are less extreme. So Claparède, Forel and Wasmann are willing to grant the possibility of a psychic life to lower animals. The physiologist Nagel was of the same mind. Jennings, who has of course psychological interests also, while not apodictic, is wholly favorable to the hypothesis. While he asserts: "There are no processes in the behavior of organisms that are not as readily conceivable without supposing them to be accompanied by consciousness as with it;" he says further: "The writer is thoroughly convinced after long study of the behavior of this organism, that if amœba were a large animal so as to come within the everyday experience of human beings, its behavior would at once call forth the attribution to it of states of pleasure, and pain; of hunger, desire and the like on precisely the same basis as we attribute these things to the dog . . . objective investigation is as favorable to the general distribution of consciousness throughout animals as it could well be."[1]

The conclusion one reaches on reading the opinions of these workers in the field of animal behavior, whether their viewpoint be largely descriptive and mechanistic, or functional and vitalistic, is that consciousness is regarded by them as a Begleiterscheinung, an epiphenomenon, something pretty vague; a concept that biology can well do without.

For this conclusion psychologists are, I believe, chiefly to blame.

James's well-known definition of psychology is 'the description and explanation of the *states of consciousness as such*.'

Judd defines consciousness as that which 'each one of us has when he sees and hears, when he feels pleasure or sorrow, when he imagines or reasons, or decides to pursue a line of action' and adds: "One hardly knows how to find phrases in which to answer those who hold consciousness to be less real and potent than physical forces. Certainly nature has protected and conserved consciousness throughout the whole development of the animal kingdom. Certainly the world is different because consciousness has been evolved. Certainly consciousness is no less real than are its conditions, and finally consciousness is certainly much more directly approachable to the student of science than is matter."[2]

Titchener uses clearer language: "Mind is the sum-total of mental processes . . . and processes implies that our subject matter is a stream, a perpetual flux, and not a collection of unchanging objects."

[1] 'Behavior of Lower Organisms,' pp. 336 and 337.

[2] 'Psychology,' pp. 13, 62–63.

The definition that consciousness is the mind's awareness of its own processes 'we must reject,' says Titchener. 'This awareness is a matter of observation of the same general kind as observation of the external world,' and the former notion is misleading 'because it suggests that mind is a personal being instead of a stream of processes.'[1]

Between defining consciousness as a *state*, and defining consciousness as a *process*, psychology has neither itself been clear as to the nature of consciousness, nor given a workable definition to students of animal behavior. In consequence such men as Bethe, Beer, von Uexkuell, men who are distinctly physiologists and have no immediate psychological interests, see in the concepts of psychology only an illusive subjective nomenclature that is both inadequate to throw further light on their strictly biological problems, and, what is more, is directly confusing. To ask whether animals have conscious *states*, whether they reflect upon their own processes as they occur, is an irrelevant, because an inconceivable hypothesis. Physico-chemical explanations, while not yet illuminative of life phenomena as such, have a very direct answer to the question of fixed behavior as the expression of life phenomena. So, for instance, tropisms are reducible to the same physical reagents that cause oscillations, tensions, and mutations in chemical processes, digestion and so forth. A fly crawls toward the light, or warmth, or the odor of decay, in a perfectly predictable way. To postulate an accompanying conscious state, does not add, but rather subtracts from our conception of its behavior.

Now much if not most of this confusion will be obviated if psychology will remain consistent to the position stated by Titchener, that consciousness is a *process*. Consciousness is a process in precisely the same sense that osmosis or alimentation is a process.

The writer believes that this position will be clarified when we overcome what amounts to a real handicap in our present psychological terminology. Let us use the term *consciousizing* for this process, and relegate the substantive term consciousness or conscious state to the realm of pure concept. The gain in conciseness will at once appear, if we now ask, not are animals conscious, but does their behavior indicate *consciousizing*. We know that metabolic processes are constantly occurring in the organism; are some of these processes consciousizing processes? And by a consciousizing process we shall mean not any sort of immediate reflection upon the inner life of process and change, but that process and change itself in so far as it involves a reference to the past experience of the animal, and a modification

[1] 'A Text-book of Psychology,' pp. 16 and 18.

of otherwise rigid behavior in terms of that experience. In this latter case, processes that ordinarily have a simple chemical or physical explanation are obviously inadequate: reaction is to a present stimulus *plus*, and this refined physiological process, this physiological process *plus*, may properly be described, I believe, as a *consciousizing process*. Reflex and mechanical it still is—the dichotomy has never properly been *conscious* vs. *mechanical*, but always *conscious-mechanical* vs. *unconscious-mechanical*—but the differentium is this sensitivity to past processes, with the result showing in a modified, more closely adaptive behavior. Once assimilated, consciousizing processes will lapse in favor of purely physiological non-consciousizing processes until such time as new needs arise demanding again the application of old experience to their solution.

Behavior will then have a three-fold description, whether phylogenetically or ontogenetically considered. We shall speak of *pre-consciousizing*, *consciousizing*, and *consciousized* behavior. Pre-consciousizing behavior will be the purely reflex mechanism in action. Infusorian life is largely, if not wholly such. Consciousizing behavior will be characteristic of all races and individuals where development occurs. Such stages are conspicuous in that branch of the zoölogical tree from planarians to man, increasingly exemplified with increase of nervous tissue and integrative mechanisms. Lastly the consciousized organism will be marked by relatively rigid, habitual, instinctive, non-progressive behavior. Such consciousized behavior the hymenoptera of all animals best illustrate: a remarkably efficient but rigid community life, with however, if we be allowed speculation, significant traces of past consciousizing processes, that, having accomplished success in adapting the organism to its environment, have now lapsed from disuse, leaving a splendid monument to their efficiency in an intricate social life—chains of physiological reflexes so rigidly conforming to type, as to exhibit little or no trace of the original consciousizing process.

These distinctions appear even more significant when applied to human consciousness. While it is probably true that pre-consciousizing behavior cannot absolutely be asserted in so actively developing an organism as the human species, nor on the other hand can we point to absolutely consciousized behavior, yet the differences between individuals in this respect, and indeed the differences in a single individual, viewed at different times in his life history, are sufficiently marked. The man in relatively quiet, stable environment lives a non-consciousizing existence for the most part. His vocabulary is

limited to less than a thousand words, his needs are few and to them he becomes early habituated. New ideas, new problems, new motor expressions are rare with him, and when presented, are either ignored, or crudely met, because of lack of that apperceptive mass of past experience sufficient to allow consciousizing, *i. e.*, assimilation of the new by the old. The recluse or rustic in the midst of busy city life is the stock illustration. The urbanite is, other things equal, a consciousizing individual par excellence. With the latter, streets must be crossed, traffic dodged, business appointments kept, while sounds, sights and disturbances of one sort or another bombard his nervous system from morning till night.

In the individual, childhood years are the pre-consciousizing years. With adolescence and its problems the consciousizing process is at its maximum, making way finally in adult life for a body of habits and fixed reactions, that demand less and less attention, and so far as they thus become rigid, are properly described as consciousized behavior.

To substitute the neologism 'consciousizing' for the conventional 'conscious state' will not solve the problem of what the process itself is. We have not, and cannot explain the consciousizing process. Neither can we explain a physico-chemical process. Why hydrogen combines as it does with oxygen to form water; why red is red and not green; why protoplasm is irritable anyhow, these are ultimate questions whose answer is locked up with the secret of life itself. Our knowledge here is profound, and a new definition is no adequate explanation. When we can understand how life goes on, what the processes of anabolism and katabolism are, we may expect to find that we have at the same time solved the question as to what the consciousizing process is. At present we can detect it, not explain it. As an organism learns, improves, profits by past experience, we are justified in asserting that it is undergoing a consciousizing process; that its past experience is being in some effective way, brought to bear upon the solution of present stimuli.

Consciousizing is then a remedial process, aroused only in the presence of some need, some ill-adjustment of organism to environment, acting against an obstacle or inhibition, and its success is marked by the disappearance of both problem and process. Its biological explanation is the inadequacy of purely physiological processes with their fixed mechanisms, to solve new situations. Their paths are canalized and rigid. Its neural basis is a plastic protoplasm, that is more than a vehicle, rather a store of potential energy, guiding

and influencing by facilitations and inhibitions the stimuli that set it off and that are the occasion of its functioning. It is better described as focalized memory than as focalized attention.

Present-day psychology is inconsistent, and so far biologists have just grounds for reproach, when it describes its chief method as introspection, in the ordinary connotation of that term. If we agree that to define consciousness as the mind's awareness of its own processes, is untenable; introspection as a psychological method becomes a misnomer. Self-observation can consist only in viewing my own immediate past instead of yours, or instead of some external fact or thing. My present consciousizing process can become aware of itself only when codified, to so speak, within the mass of past experience. It may then present itself, and does do so, as a problematic object for a still later consciousizing process, the latter as yet unaware of itself. The current uncorrected notion that we can grasp the momentary experience as it passes is the arch illusion which contemporary psychology allows itself.

The concept of consciousness, or the concept of the self remain unimpaired. They are as valuable and as unreal as the electron of physics. Such an entity as the self or as the electron may exist. To postulate them as concepts is both possible and wise; to treat them as real things of mass and objectivity is confusing. Of such concepts biology and physiology have no need. Behavior can be everywhere explained without invoking conscious selves. But neither biology nor physiology can dispense with consciousizing processes which are as real and as universal as are growth, or development, or evolutional processes.

By strict adherence to this dichotomy of pure concept on the one hand and process on the other, a position not only creditable but alone possible for a self-consistent psychology, physiology will no longer feel itself called upon to build up an artificial system of new terms to explain behavior. Such a make-shift as that attempted by Bethe, Beer, and von Uexkuell, is as unfair to physiology as it is to psychology. Loeb will no longer call consciousness a metaphysical term, and we shall accept his 'associative memory'[1] as a fair synonym for 'consciousizing process.' With von Uexkuell, who in his latest paper attempts to make provision for a modification of the machinery of behavior through the experiences of the organism, and who builds up his explanatory system about the

[1] 'Comparative Physiology of the Brain and Comparative Psychology,' pp. 12–14, 214 et seq.; 236 et seq.

concept of tonus, we shall find ourselves in no necessary disagreement. Of tonus he says: "Es stellt sich der Tonus als eine Energieform dar, die . . . immer vom Orte hoeheren Tonus zum Orte niederen Tonus abfliesst." Further, it is ". . . denjenigen Theil der Lebensintensitaet der einzelnen Zelle der . . . dem Gesammtorganismus zur Verfuegung steht."[1]

One may still preserve his psychological integrity and with Bethe agree: "Jede Association ist Folge einer eben vor-aufgegangenen Errinnerung; jede Errinnerung Folge einer voraufgegangenen Wahrnehmung; jede Wahrnehmung verursacht durch einen aeusseren Reiz, und so ist auch die letzte Schlussfolgerung einen langen Gedankenreihe mechanisch verursacht durch einen aeusseren Reiz. Unvermittelt auftauchende Errinnerungen und Gedanken giebt es nicht. Ohne aeusseren Reiz ist auch. . . . Errinnerung und Gedankenarbeit unmoglich. . . ."[2]

Our consciousizing processes could be so characterized. Energy is stored in some modified fashion by past experience; it is put in action by the stimulus now affecting the organism; its result is to modify the machinery of behavior in terms of that past experience. This is what and this is all psychology can mean by conscious *processes*.

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[1] 'Die anatomische Elemente des Nervensystems u. ihre physiologische Bedeutung,' *Biol. Cent.*, Bd. 18, p. 868.

[2] 'Die Wirkung von Licht u. Shatten auf die Seeigel,' *Zeit. f. Biol.*, Vol. 40, p. 474.

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